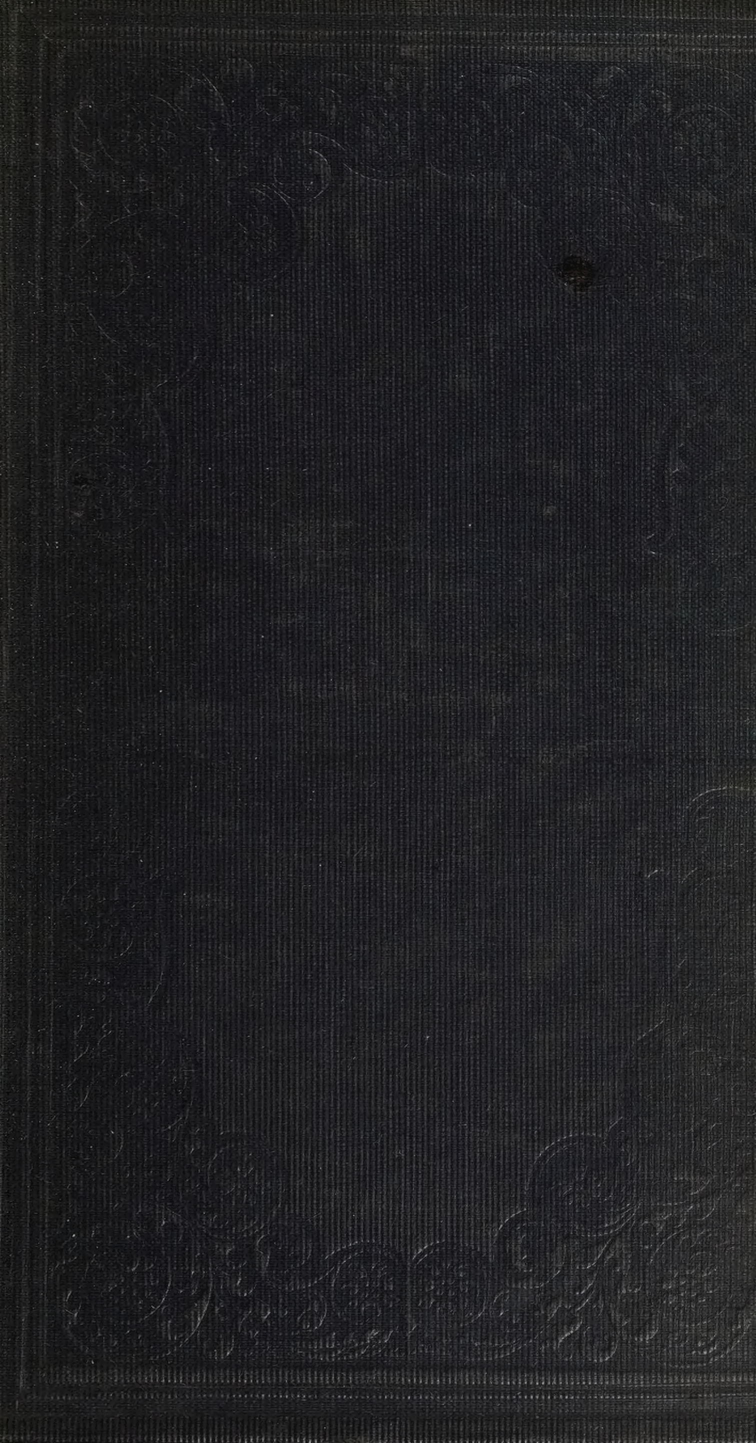

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BRAITHWAITE'S RETROSPECT.

VOL. LVI. JULY—DECEMBER, 1867.

THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND
PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN
AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

JAMES BRAITHWAITE, M.D. LOND.

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SYNOPSIS,

(ARRANGED ALPHABETICALLY), CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR; INCLUDING SEVERAL PAPERS NOT INSERTED IN THE BODY OF THE WORK.

AFFECTIONS OF THE SYSTEM GENERALLY.

CANCEROUS AND OTHER TUMOURS.—*Internal Use of Lime.*—Dr. Peter Hood, of London, gives an account of a case of cancerous tumour of the breast, which entirely separated, leaving a raw granulating surface, this result being attributed by the patient to the internal use of small quantities of lime from the inner surface of oyster shells. This she had taken in consequence of hearing of a case which had been cured by the same treatment. The *modus operandi* suggested is, that ossification of the arteries of the tumour occurs, whereby its nutrition is cut off, and its death ensues. This mode of treatment we would suggest is most likely to be of use in the slowly-growing tumours of persons of advanced age. Previously to scraping out the "small white" part of the interior, the shells require baking for three nights in a slow oven, and the dose recommended is as much as will lie on a shilling once or twice a-day. (Lancet, Oct. 12, 1867, p. 454.)

CHOLERAIC AND ZYMOTIC POISONS.—*Nature of.*—Dr. Halford, of Melbourne, has discovered that by the bite of a cobra-di-capella molecules (cellular) of germinal matter are thrown into the blood, and by their rapid multiplication destroy life. This is not theory, for they can be detected without much difficulty, and examined microscopically. They are circular cells, one seventeen-hundredth of an inch in diameter, and containing a round nucleus, and other still more minute spherules of living germinal matter. He says, "I have many reasons for believing that the materies morbi of cholera is a nearly allied animal poison." If so, may we not hope to know something definite of the poisons of hydrophobia, small-pox, scarlet fever, and, indeed, of all zymotic diseases? (British Medical Journal, July 20, 1867, p. 43.)

FEVER.—*Only one Fever Poison.*—There is only one fever poison, and the forms of fever known as typhus, typhoid, and relapsing, are only varieties of one disease. The author has had extensive opportunities of observation, and his arguments in support of his opinion are well worth perusal. (Dr. H. Kennedy, p. 2.)

STIMULANTS.—*Abuse of.*—Of course stimulants are often needed in cases of fever, but young persons with typhus and typhoid do far better, I believe, without them. That they make good recoveries on simple milk diet is a fact which no argument can gainsay; and on the other hand a marked improvement takes place in some cases where a stimulus has been left off. In bronchitis I have repeatedly seen improvement after stimulants have been omitted. As regards heart disease, I am convinced that the amount of mischief done by stimulants is immense, and that the utmost discrimination is required in their use. There are cases where an undoubted benefit is conferred by them; and there are others where alcohol has induced palpitation, fluttering, great distress, and constant sleepless nights, and where, on the other hand, the withdrawal of the spirit, and the substitution of a dose of digitalis or henbane, has been of the most essential service. (Dr. S. Wilks, p. 31.)

TYPHOID FEVER.—*Enlargement of the Spleen.*—Typhoid fever often commits the greatest ravages among young people in a guise not at all times easily detected, for the eruption is far from constant. There is, however, one clue of the greatest importance in the diagnosis of such doubtful cases. This is enlargement of the spleen, which, if it is found, may enable us to decide that typhoid fever is present. (Dr. Jenner, p. 1.)

Headache in Typhoid Fever.—If headache prevail throughout this disease it is due to cerebral mischief, and not to the fever itself. (Dr. Jenner, p. 1.)

Opium in Typhoid Fever.—When certain symptoms are present in a case of typhoid fever, we must not be afraid of administering opium. It is required when there has been insomnolency for a long period, combined with exhaustion and a peculiar pulse. That character of pulse is hardly to be described, but it is extremely rapid, and difficult to be counted because of its weak, fluttering, and indistinct character. The beats appear to run into one another, without that appreciable interval present in all other variations in the pulse, whether in health or disease. An interesting case is related, in which a profound slumber resulted from the administration of half a grain of opium, and recovery from an almost hopeless state ensued. (Dr. N. G. Mercer, p. 16.)

TYPHUS FEVER.—*Stimulants.*—Every case of fever must be treated on its own merits, but as a rule old people require stimulants, whereas young persons do not; the danger in the first being from exhaustion, in the second from the head symptoms. Every now and then a case occurs in young people in which free stimulation is required, just as in some cases in elderly people it will do harm. It is folly to compare one plan of treatment statistically with the other, no general rule can be laid down. (Dr. T. J. Maclagan, p. 14.)

The action of alcohol in fever is two-fold, stimulant in small doses and narcotic in larger. A fever patient is much more sensitive to both these actions than a person in health. When along with a frequent pulse and a weakly acting heart, there is sleeplessness or delirium, alcohol may be employed as a soothing agent with more benefit and safety than opium. Two ounces of whiskey made into toddy may under these circumstances be administered, with the effect of inducing a comfortable sleep, after opiates have completely failed. The relief to the delirium found by Dr. Todd to result from the continuous administration of brandy was owing to narcosis, and this continued for days cannot but be injurious. (Dr. J. B. Russell, Glasgow Medical Journal, Oct. 1867, p. 205.)

AFFECTIONS OF THE NERVOUS SYSTEM.

DELIRIUM TREMENS.—*Capsicum.*—Give a dose of twenty grains of capsicum in a bolus, as early as possible, in cases of delirium tremens. It produces a most powerful stimulant and sedative effect. A case is related in which the patient exhibited tremor in almost all the muscles of the body, chilliness, debility, sleeplessness, foul tongue, severe and general uneasiness, but a total absence of illusions, horrors, or delirium to any degree. The disease appeared to be at once cut short by the treatment here recommended. This case is an instance of that form and stage of the affection which will most probably be benefitted. (Dr. Lyons, p. 80.)

EPILEPSY.—*Strychnine.*—In strychnine we possess a drug which will always control the excitability of the medulla oblongata and restrain the attacks of convulsion. Large doses of the drug must however be given to produce the favourable results. In some cases it has been carried as high as a fifth of a grain twice daily, and this continued for nearly three weeks at a time, not only without its producing the slightest sign of irritation, but on the contrary the most marked diminution in the frequency and violence of the attacks. The one should be commenced at one-sixteenth of a grain twice daily, and gradually increased according as it is tolerated. It is remarkable, however, that in these cases the system appears to lose its susceptibility; and the drug, even in large doses, produces some of the ordinary signs of disagreement. In no case has it yet been seen to produce mischievous excitement or irritation. (Mr. W. Tyrrell, p. 44.)

NEURALGIA.—In all cases of neuralgia there is more or less acute pain on pressure over the spinous processes of those vertebræ which correspond to the origin or point of exit of the affected nerves. When neuralgia is superficial the local application of atropine or belladonna is sufficient to relieve pain in the majority of cases. A piece of lint soaked in a solution of the salt (5 grains to 3 ounces) applied upon the painful part, and covered with oiled silk, is the best mode of applying the remedy. When, however, the neuralgia is more deeply seated or severe, the subcutaneous injection of morphia or atropine is necessary. (Prof. Trousseau, p. 91.)

PAIN.—*Morphia and Chloroform.*—The following combination of morphia and chloroform is said to give relief in most painful affections much more quickly and certainly than morphia alone, and to leave none of the unpleasant after-effects of opium. Dissolve one part of morphia by weight in two parts of rectified wine-vinegar, and twenty parts of rectified spirit of wine; the solution when cold being mixed with eighty parts of chloroform. One drop contains the three-hundredth part of a grain of morphia. The dose for a child is two to fifteen drops; for an adult, thirty to forty drops. (p. 400.)

PARALYSIS.—*Cold to the Spine.*—Cold applied directly to the spine is of much benefit in cases of paralysis in which there is reason to suppose that the spinal cord is in an unhealthy and excitable, though perhaps exhausted, state. The cold may be applied by means of cloths dipped in iced-water, or by ice-bags. In the first case related, and this may be taken as a typical one, the disease originated by injury to the hand, and a morbid influence was thence propagated to the spine. The symptoms at the commencement of treatment consisted of paralysis involving the muscles of the tongue, throat, back, and four extremities, and associated with giddiness, headache, profuse dribbling of saliva, constipation, and scantiness of the menses. (Dr. J. Chapman, p. 66.)

RHEUMATIC PARALYSIS.—*Electricity.*—As a rule ordinary routine medical treatment has no effect upon rheumatic paralysis, and the disease progresses from bad to worse, until the use of the limb or limbs is completely lost. Yet of all the diseases which come under the treatment of the medical electrician none is more amenable to the judicious application of electricity. It will often be found that the muscles have lost their contractile power to a considerable extent. Daily, for half an hour, each muscular fibre of the paralysed muscles should be excited with the inverse current of the primary wire of the electro-magnetic battery. All the rest of the twenty-four hours a Pulvermacher band should be worn. The positive pole should be placed on the spine, and the band should then be wound round the paralysed part, and the negative pole placed on the lower part of the principal paralysed muscle and retained in position by an elastic bandage. The band should be excited twice a-day with one part vinegar and eight parts water. This treatment must be carried on without change as long as necessary; in the case from which this plan of treatment is taken three weeks was necessary. (Mr. H. Lobb, p. 77.)

Paralysis from Sciatica.—A case of severe sciatica of three months' duration is related, in which the leg was useless and the patient worn out with pain and fatigue. The positive pole of a Pulvermacher 120 element battery was applied to the lower portion of the spine by means of a wet conductor, and the foot was placed in a basin of water brought into the current with the negative pole. This current was allowed to pass for about twenty minutes, when two No. 0 bands were put on the positive pole on the lower portion of the spine, and, taking a turn round the leg, the negative pole was placed beneath the sole of the foot, the bands being excited with one part vinegar to eight parts water, and retained in position by means of an elastic bandage. This plan of treatment was most successful: it generally is so in such cases, all that is required being the passage of a continuous galvanic current through the nerve in large quantities. (Mr. H. Lobb, p. 78.)

TETANUS.—In the present state of medical knowledge our greatest chance of curing tetanus and tetanoid affections consists in the proper application of ice. It is of the utmost importance that the ice should not only be applied to the spine, but that a bag containing it be applied directly across the occipital region in which is contained the most important part of the cord. Everything depends upon the way in which it is applied; and the non-success of some who have tried and reported upon this plan of treatment probably depends upon their not carrying it out properly. (Dr. J. Chapman, p. 24.)

Traumatic Tetanus.—Calabar Bean.—A case of severe tetanus is related in which, during 36 hours, about six grains of Calabar bean were given. The patient then became half comatose; pupils considerably contracted; breathing more easy; and no tendency to opisthotonos. The muscles of the extremities became quite relaxed, and she had a feeling of sinking, as if about to die. Brandy had to be administered. Sleep followed for the first time since the commencement of the illness. Next day the patient was much depressed, pulse 120, fuller than it had been; pupils slightly contracted; breathing easier; muscles of the arms and legs quite flaccid, although unable to move them. When moved, she had still severe spasms of the muscles of the back. The medicine was continued in small doses for a short time longer, and recovery followed. "There can be no doubt that it has a wonderful effect in producing relaxation of the tetanically contracted muscles." (Dr. A. Campbell, p. 48.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

ACUPRESSURE.—Upwards of eight hundred vessels have now been stopped by acupressure by the surgeons to the Aberdeen Infirmary. These have been in all kinds of serious operations, and yet in only two has there been hemorrhage on removal of the needle. One of these was after removal of the leg in its upper third for medullary cancer; the second was the radial artery, hemorrhage from which was arrested at once by replacing the needle. This is quite conclusive as to acupressure being a perfectly reliable means of arresting hemorrhage. Dr. Pirrie says: "I am perfectly convinced that acupressure accelerates the healing of wounds; and that under its use, aided by metallic sutures, the avoidance of all dressings, and perfect rest of the wounded part, in many instances the largest wounds, after major operations, will heal up without the formation of a single drop of pus." Add to these recommendations that acupressure is the easiest of performance and the quickest of all methods of arresting hemorrhage, and that the risk from pyæmia is diminished very greatly (no case having yet occurred); and impartial men will allow that the use of the ligature should be superseded almost entirely by that of the needle. The details of the various modes of applying the needle are given most fully. (Dr. W. Pirrie, Sir J. Y. Simpson, pp. 232, 241.)

Removal of the Acupressure Pins.—It is quite unnecessary to leave the pins *in situ* for any length of time. In one case the axillary artery was relieved from pressure in fifteen hours, and the spermatic arteries in two hours and a half in another case. The most remarkable case was that of a boy, who removed three of the needles four hours after amputation of his thigh, one of which had compressed the femoral artery;

no hemorrhage resulted, the artery being completely closed. The pins used should have glover's points, as they are less likely to cut the tissues. (Dr. Pirrie, p. 246.)

ANEURISM.—*Complete Arrest of the Current through the Sac.*—It is quite a mistake in the treatment of external aneurism to merely check the current. This plan has only succeeded in 62 per cent. of the cases in which it has been tried, and is, moreover, attended with much suffering to the patient. The author has lately treated four cases by completely arresting the current in the sac; at the same time, by compressing the artery at the distal end of the aneurism, retaining the blood in the aneurismal sac so as completely to distend it. The distal pressure should precede the proximal, in order to ensure the distension of the sac. In popliteal or ante-cubital cases, distal pressure being impracticable, Mr. Hart's flexion plan may achieve the same object. He believes that cases are never cured by partial compression, and that in those cases said to be cured by that means, the pressure having been increased to the amount of complete arrest for a short time, clotting in the sac, or plugging of the artery below it, produced the cure, for the pulsation is usually reported to have suddenly ceased. In the case of popliteal aneurism the proper way of proceeding is to raise, bandage and flex the limb, and then compress the femoral artery so as to completely arrest the circulation through it. (Dr. E. D. Mapother, p. 217.)

Aortic Aneurism.—Galvanism.—In a case of aortic aneurism threatening death by external hemorrhage, the author introduced into the sac two needles, coated the one with glass and the other with gutta-percha in the principal part of their length, and a galvanic current was by their means passed through the blood contained in the part of the sac of the aneurism external to the chest walls. No result following this operation, it was again repeated, the needles being this time coated with vulcanite, and six cells instead of four being used. In the first operation the current was continued seventeen and in the second twenty minutes. The case ultimately proved fatal, and it was found at the post-mortem examination that a considerable quantity of blood had undergone coagulation. This case seems to prove that we have a remedy by which we can at least solidify the external portions of an aneurismal sac, and that by frequent and longer-continued operations we may fill by coagulum a sac of any size. Success would depend principally upon the size of the opening into the aorta. If large, the clot formed would become free on withdrawal of the needles, and would pass on to plug up the aorta or one of its branches; but if the aperture is small, complete consolidation might be produced. (Dr. J. Duncan, p. 223.)

Cirsoid Aneurism.—Injection of Perchloride of Iron.—Cirsoid aneurism, even of large size, may be cured by injection of the perchloride of iron. A case treated in this way was lately exhibited before the Medical Society of Vienna. The tumour had been large, and situated on the side of the head. All the arteries leading to it were much dilated. After the injection, a clot was formed, the part became hard, suppurated at points, and the tumour disappeared. (Prof. Pitha, p. 231.)

ANGINA PECTORIS.—*Nitrite of Amyl.*—Nitrite of amyl diminishes the contractility of muscles and lessens arterial tension. This latter property has been proved both by the sphygmograph and the hæmadynamometer. In a case of very severe angina pectoris which there was lately in the clinical wards of the Edinburgh Royal Infirmary, the only treatment which afforded relief was venesection. The relief so obtained was, unfortunately, only temporary, and apparently arose from the relief to arterial tension. Chloroform also gave relief, but the symptoms recurred immediately on its discontinuance. Under these circumstances five to ten drops of the nitrite of amyl were given by inhalation, and simultaneously with the flushing of the face the pain completely disappeared, and did not return till its wonted time next night. The same remedy gives relief in cases of aortic aneurism when the pain is not constant, but intermitting. Dr. Richardson thinks it may prove useful in tetanus, and it also seems likely, from its mode of action, to be beneficial in some cases of asthma—possibly also in cholera. (Dr. T. L. Brunton, p. 104.)

HYPERTROPHY AND DILATATION OF THE RIGHT VENTRICLE OF THE HEART.—The apex beat is an indication of the state of the left ventricle, and is the only healthy impulse perceptible to the hand placed on the chest. In cases of hypertrophy and dilatation of the right ventricle, with a suppressed or a relatively diminished left apex beat, we have a diffused impulse to the right of the usual position of the apex beat, and commonly easily distinguished from the latter (even when both are present) by its diffused and heaving character, readily appreciated by the hand strongly pressed over the costal cartilages. Sometimes there is no difficulty in detecting a proper apex beat in its usual situation, or perhaps in such a position as to indicate hypertrophy of the left ventricle; and, in addition, there is a diffused flat impulse, heaving up the whole palm of the hand, quite distinguishable from the other, and extending from the third or fourth left cartilage almost to the xiphoid cartilage, or into the epigastrium. In such a case it is to be presumed that both ventricles are very considerably enlarged; but the relative degree of enlargement of the right or left ventricle will be determined mainly by the degree to which the apex beat retains or loses its definite character. (Dr. W. T. Gairdner, p. 92.)

Ligature of the Femoral Artery in Porter's Space.—The following is the manner of securing the femoral artery in this situation:—A transverse incision, about two inches in length, parallel with, but half an inch below, Poupart's ligament; the incision being so effected as to leave the artery in its centre. The method of performing this incision is simple in the extreme. A fold of skin is pinched up—the operator himself holding one side of the fold, his assistant the other; and it is transfixed with a sharp-pointed bistoury; and, by cutting outwards, the first incision will be completed. The most ordinary care will ensure the artery's being in some portion of the wound. A very few touches of the scalpel, with the assistance of a director, will now expose the vessel; and it can be secured in any way that the operator may select. The great advantage of this operation is that, in this situation, the artery is widely separated from its vein; and, consequently, there is no danger of wounding it. The facility with which the artery is reached is also a recommendation. When the femoral artery is tied above the profunda the current of blood finds its way through the anastomoses existing between the obturator and the internal circumflex arteries; the ilio lumbar with the circumflex ilii; and others of the gluteal, sciatic, and pudic. (Dr. R. Macnamara, p. 253.)

Moveable Forceps for Closing Bleeding Blood-Vessels.—This is a new method of closing arteries after operations, by the application to the vessel of a pair of self-closing forceps, something like a *serre-fin*, but having the toothed extremities at the end of long firm shanks, which, when closed together, are but little thicker than a stout needle. Various sizes are required ranging from three inches to three-quarters or half-an-inch long. The forceps are self-holding; to open them it is only necessary to press upon the bow at the top. The points of the forceps should just embrace the vessel, while the bowed shoulder should just project out of the wound. If venous hemorrhage be troublesome, there is no reason why the forceps should not be applied to the open vein, as well as to a bleeding artery, for the effects would not be like those to be feared from a ligature, and the forceps may be removed on the completion of the operation or very shortly afterwards. By the use of these forceps all the advantages of acupressure are gained without the risk of secondary hemorrhage, which sometimes occurs and involves the opening out of the wound, and without including in the compressed portion any other tissue than the open end of the vessel. (Mr. T. Nunneley, p. 248.)

NÆVUS.—Whatever plan of treatment is adopted it is essential that it destroy the whole growth completely. When the nævus is prominent and cutaneous the ligature answers the purpose the best. Pass two harelip needles at right angles to each other under the nævus, and strangulate the growth by a ligature passed around it beneath the ends of the needles. The use of the needles is of course to ensure the ligature being applied beneath the base of the tumour. Prominent cutaneous nævi when irregular in shape or ill-defined in their boundaries are best attacked by nitric acid, as this can be applied with a glass brush to the very smallest spots of diseased tissue without injuring the surrounding skin. "Anhydrous" acid is the best for this purpose. Vienna paste, or caustic potash is used by many for this kind of nævus, but it is difficult to adapt them to the irregular edges and outlying portions of these growths. (Mr. T. Smith, p. 262.)

Nævi on the Face.—The treatment of nævi on the face by the injection of perchloride of iron has more than once been followed by instant death. A case was published by Mr. Paget, of Leicester, in 1837, in which a slight convulsion and death resulted instantaneously from the injection of a small quantity of dilute liquor ammoniæ. Nævi over the fontanelle also require treating with great caution. It is always better to wait until ossification has taken place beneath the nævus before doing anything. Death from the injection of perchloride of iron results from its introduction into the general circulation, by which untoward accident coagulation of the blood in the vein and in the venous system down to the right side of the heart is effected. (Mr. T. Smith, p. 267.)

PULSE.—*A Rapid Pulse* does not alone necessarily indicate mischief. There are a considerable number of children and young persons whose pulses are rapid enough to frighten one. Observe whether the respirations are in the same proportion rapid; if they are not, the respirations and not the pulse must be our guide in judging of the patient's state. After hemorrhage we must always apply this rule of checking the indications of the pulse with those of the breathing, for the pulse is usually hastened and the breathing retarded; so that with a pulse of 120 or more there may be not 10 respirations in the minute. (Prof. Paget, p. 208.)

PUS GLOBULES AND WHITE CORPUSCLES OF THE BLOOD IDENTICAL.—Dr. Cohnheim, of Berlin, has made some remarkable observations proving the identity of the white corpuscles of the blood with pus globules. White blood corpuscles have the singular property of contractility, fixing finely divided substances with which they come in contact in their contractile stroma. Dr. Cohnheim, taking advantage of this, injects anyline-blue into the blood of an animal, whereby the white corpuscles become coloured, and he finds the pus globules evolved on the surface of an artificially inflamed cornea, to present the exact appearance of the coloured white corpuscles. The white blood corpuscles are in fact the cellular elements infiltrating the inflamed part. (British Med. Journal, June 22, 1867, p. 740.)

TORSION OF BLOOD-VESSELS.—Mr. Syme in many operations especially in breast cases, does without ligatures, needles, &c., to stop the vessels. He simply trusts to torsion used as follows: He seizes the vessel with the hooked forceps, or the common artery forceps, and twists it round and round till the little bit held by the forceps separates or is twisted from the vessel. (p. 172.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

BRONCHIAL AFFECTIONS AND ASTHMA.—*Inhalation of Ammonia.* —Ammonia inhaled is a remedy of considerable value in diseases of the respiratory mucous membrane, and in asthma. In the latter disease when the vital powers are depressed it produces a feeling of warmth and exhilaration particularly grateful to the patient. The best plan of employing it is that of Dr. Lewin. The apparatus consists mainly of three bottles, as used in chemistry for the evolution of gases, one containing some liquor ammonia, and another containing some hydrochloric acid, according to their chemical proportions, are connected with a third, which is about half filled with pure water. From the last bottle the inhaling tube projects. The patient uses the mouthpiece of the inhaling tube exactly as a pipe is used by a smoker. With each draught nascent chloride of ammonia is inhaled and carried into the smallest ramifications of the bronchial tubes. (Dr. H. Beigel, p. 125.)

CONGENITAL DISPLACEMENT OF THE SEPTUM NASI SIMULATING POLYPUS.—Congenital displacement of the septum nasi to one side so as nearly to occlude one nostril is a very common affection, and is liable to be mistaken for polypus. A careful examination will prevent this. The nose appears quite straight outside. There is usually a fœtid discharge from the occluded nostril owing to retention of the mucus; this may be cured by means of tepid water and a syringe. (Mr. T. Bryant, Lancet, Aug. 24, 1867, p. 224.)

DISEASES OF THE RESPIRATORY PASSAGES.—*Inhalation of Atomised Fluids.*—There can be no doubt but that atomised fluids when inhaled enter the respiratory tract and penetrate into the very cells of the lungs; and that therefore by means of inhalation remedies may be most appropriately and successfully applied to the organs of respiration. Dr. Beigel, of London, in a work lately published on the subject, gives woodcuts of and describes various atomising apparatus, the acting blast power of some of which is steam. Among the medicaments which can be atomised, and which in that condition have been found beneficial in diseases of the respiratory organs, are nitrate of silver, tannin, sesquichloride of iron, common salt, tincture of opium, and cod-liver oil. (Dr. H. Beigel, p. 128.)

OZŒNA.—The anatomical fact that, when the mouth is opened the velum pendulum palati closes, so as to shut off the nasal cavity, may be made use of in the treatment of ozœna, (as was first pointed out by Dr. Thudichum, see *Retrospect*, Vol. li. p. 196). The nozzle of a tube, the other end of which is placed in a vessel of water raised considerably above the patient, should be placed in his nostril—not the one which it is desired to free from matter or mucus. The tube will of course act as a syphon if filled with water previous to use, and a constant stream of water will pass in at the healthy nostril and out at the other, carrying with it any offensive discharge or mucus which may be present. (Mr. Christopher Heath, Lancet, July 13, 1867, p. 40.)

PHTHISIS.—*Inoculation of Animals as a Means of Diagnosis.*—For the remarkable discovery that tubercular phthisis can be inoculated from man to animals we are indebted to Dr. Villemin. It having occurred to the author that this might be made a means of diagnosis in cases of doubtful phthisis in the humun subject, many experiments were made, and the following conclusions arrived at. That the inoculation of guinea pigs with the expectoration of phthisis will, at any rate in a certain stage of the case, produce tuberculosis in the animal, and if two or more animals be inoculated at a time, and on being killed thirty days afterwards are found to be tuberculized, we may be certain that the patient is suffering from phthisis. Blood and pus as well as the expectoration seem to have the power of causing the formation of tubercles in guinea-pigs when inoculated in these animals. The spleen is the part in the guinea-pig (the most suitable animal for the experiment) which is most liable to tuberculosis. (Dr. W. Marcet, p. 115.)

Phthisis a Zymotic Disease.—The author believes that tubercle is a true zymotic disease, and that it never originates spontaneously. He considers that the tuberculous matter contains, or is, the morbific matter of the disease, and is of the nature of an eruption, and that there is reason to hope we may eventually rid ourselves entirely of this fatal scourge by the destruction of this matter on its issue from the body. Amongst the evidence on which these conclusions are based one of the most curious points is the fact that phthisis was unknown amongst the South Sea Islanders, the American Indians, and the Negroes of Africa, until they have come into intimate contact with Europeans. According to the positive assurance of Livingstone and other African travellers, phthisis is unknown in the interior of Africa, whilst it is extremely common and fatal everywhere along the African seaboard, where the blacks have come into constant and intimate relation with the whites. (Dr. W. Budd, p. 112.)

Influence of Alpine Climates on Pulmonary Consumption.—Some startling cases are related showing the beneficial influence of Alpine climates on pulmonary consumption. One patient lost his cough and gained flesh on the Peruvian Andes, and, after a subsequent relapse at New Orleans, entirely recovered his health on the table-land of Mexico. A second patient after spending a winter at Bordeaux and Cannes without entirely losing his cough, and where he still continued to lose weight, went to the Righi in Switzerland, where he lived almost entirely in the open air and drank about two quarts of milk a-day. He recovered his health perfectly so far as his own sensations went. The third case went to Davos am Platz and recovered. The treatment adopted there by Drs. Spengler and Unger, is the use of much milk and light nourishing food; a moderate amount of wine, principally the red wine of the Valteline; and graduated exercise, first on level, later up hill. The cold douche is likewise in many cases used with advantage. It is probable that in addition to the exhilaration and increased inclination to exercise produced by the cooling refreshing atmosphere and the complete change in the manner of living, that the increased exercise of the respiratory organs, the fuller, deeper inspirations necessitated by the rarity of the atmosphere influences the respiratory circulation and all the processes of nutrition. (Dr. H. Weber, p. 118.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

ARTIFICIAL DIGESTION OF MEAT.—There are many cases in which it is very important that nourishment should be supplied in sufficient quantity to the system, yet in which the stomach is incapable of fulfilling its functions aright. Beef-tea and milk are often the only articles of diet the stomach will bear, and even the latter frequently disagrees, whilst beef-tea alone does not supply sufficient pabulum. In such cases let the meat be digested artificially as follows: Add to one pound of meat well divided, 58 grains of hydrochloric acid, sp. gr. 1·1496, in a pint of water, 15 grains of Boudault's pepsine, and allow the whole to simmer over a water-bath at about the temperature of the body. When the meat is sufficiently broken up, it should be strained and the acid neutralized by 81 grains of bicarbonate of soda. The product is of a most agreeable character, easily digestible, and containing much more nourishment than common beef-tea. (Dr. W. Marcet, p. 141.)

DIPHTHERIA.—Dr. Alderson, President of the College of Physicians, disapproves of the plan of removing the false membrane from the throat, and the application of caustics. He says: "I should deprecate the use of every irritating application in the early stage; though when that gorged state of the parts, having an œdematous character, threatening immediate suffocation, has begun to subside, a certain amount of irritating applications may be admissible and even necessary." He considers that alcoholic stimulants or ammonia are injurious. General depletion in diphtheria would be wholly out of place, but he has observed much benefit from leeches cautiously applied, so as to produce only a local depleting effect. "It is not desirable to continue the depletion longer than is called for to check the inflammatory action. The tendency in this disease to extreme debility and exhaustion is very great, but at the same time it has been most strikingly evident, in all cases which I have treated in this manner, that the rebound towards strength and convalescence succeeded immediately on relief attained through the leeches." (Dr. J. Alderson, p. 129.)

Iodine Inhalation.—The author states that he has found benefit from the inhalation of iodine in cases of diphtheria. The formula he recommends is that formerly used by Sir Charles Scudamore in pulmonary phthisis, viz: iodine, iodide of potassium, of each four grains; alcohol, four drachms; water, four ounces. Of this for each inhalation, commencing, he takes a drachm, adds to it a pint of vinegar, infused with a handful of dried garden sage, placed in a common inhaling jar, steadily increasing the quantity of iodine solution until he arrives at half an ounce at each inhalation. (Dr. J. Waring-Curran, p. 134.)

Hemorrhoids.—The part to be removed is siezed with a clamp, the under surface of which, *i.e.*, that next the anus, is covered with ivory, and the instrument is prevented from slipping by a screw. The hemorrhoid, grasped by the clamp, is then cut away, and the actual cautery energetically applied to the wound, the ivory preventing the conduction of the heat, and thus saving injury to the parts behind. The screw is then gradually loosened in case of hemorrhage, which, should it occur, can be easily stopped by re-tightening the screw. (Mr. H. Smith, p. 274.)

Intestinal Worms.—*Substances Liable to be Mistaken for.*—A number of cases are related in which orange pulp, undigested celery, plastic lymphy intestinal exudation, and elastic ligamentous tissue, passed from the bowel, were severally mistaken for intestinal worms or hydatids. Care in the examination or the use of the microscope will render the nature of such substances evident. (Dr. W. Frazer, p. 273.)

Obstruction of the Bowels with Obstinate Constipation.—In cases of obstinate constipation with urgent symptoms of obstruction nothing is so reliable as a large enema of warmed oil, if the bowels cannot be relieved by simpler means. First give an ordinary enema of warm water, to which a few drops of laudanum may be added to secure its retention; then give fully three pints of warmed oil, and cause it to be retained as long as possible by pressure on the seat with a soft linen napkin. Some most interesting cases are related illustrative of this practice. (Dr. T. Head, p. 269.)

Phosphate of Soda in Small Doses.—The action of phosphate of soda is that of a mild saline aperient when administered in large doses, but it is an important alterative in small doses. It is an important salt in the animal economy, and one often sadly deficient in the food for children. Dr. Marcet has pointed out that the fatty acids are converted into an emulsion by its agency, and that they thus may become more easily assimilated. It is of much value wherever, from an unhealthy character of the motions, the visceral secretions seem to be abnormal. The parents should be directed to put a pinch of the powder into each article of food the child receives. There appears to be a similarity between its action and that sought by the administration of gray powder "in alterative doses." It is of service in cases of chalky stools or white fluid motions in infants who are being artificially reared, and who are liable to frequent derangement of the bowels. (Dr. W. Stephenson, p. 123.)

Vomiting from Sarcinæ Ventriculi.—Sulphurous acid should be made in the ordinary way, and be passed through water until the latter has dissolved up as much as it will take. This saturated solution is to be diluted until the acid is no longer unpleasant to swallow, when an ounce should be given before each meal. The sarcinæ may be thus destroyed, but are apt to return, as will other parasites, until the conditions on which their presence depends have been removed. (Dr. Reynolds, p. 141.)

AFFECTIONS OF THE URINARY ORGANS.

Bright's Disease.—The investigations of the author show that, providing the degeneration of the kidney is not fatty, a great deal can be done towards effecting a cure. The presence of albumen in small quantities (5 or 6 grains daily) still continues in the most favourable cases observed by him—but the health being good this may be disregarded. The free action of the skin must be maintained by the occasional use of the vapour bath. Congestion of the kidneys must be removed by dry cupping. Tonics and astringents should be administered, consisting chiefly of the tincture of sesquichloride of iron, sulphate of quinine, tannic and gallic acids, and when the effusion is very great, occasional doses of hydrogogue cathartics and diuretics should be given : but above and before all, great attention should be paid to the diet. This must be highly nitrogenous ; milk and eggs being freely given, and meat two and even three times a day. Rest from toil and freedom from care and anxiety are absolutely necessary. (Dr. A. H. Hassall, p. 149.)

Irritable Bladder.—In a case of extreme irritability of the bladder which lately occurred at University College Hospital, no stone existed, and the urine was perfectly natural and healthy in every respect. No cause whatever could be assigned for the complaint. The patient was a man, and he was obliged to pass urine as often as four and twenty times in the twenty-four hours. Belladonna administered as suppository and given by the mouth speedily improved his condition, but he did not get well. *Alchemilla arvensis*, or parsley-piot, a common field plant, was then given, an infusion of which (one ounce to the pint) will often succeed where more pretentious remedies have failed. (Sir H. Thompson, p. 289.)

LITHOTOMY.—Sir Wm. Fergusson lately operated on two cases of very large stone in the bladder by a modification of the operation of Celsus, making the outer cut to form a crescent or rainbow shape, the hollow of which included the rectum. The fore-finger and middle-finger of the left hand, with their dorsal surface looking up, were then directed horizontally forwards, so as at once to push back the rectum out of harm's way, and to embrace between them the bend of the staff, furnishing thus an effective guide to its groove, which the knife then entered, and completed the operation exactly as for lateral lithotomy. (p. 284.)

Mr. Lee has recently introduced a modification of the lateral operation for stone. After passing an ordinary grooved staff a straight median incision was made, and continued from just in front of the anus outward and backward so as to embrace about one fourth of the circumference of the bowel. The scalpel was then re-introduced in the median line, with its back towards the rectum; it was then passed forwards into the groove of the staff, and the membranous portion of the urethra having been opened it was withdrawn, and a curved bistoury with a projecting probe-point introduced. The probe-point was then made to slide along the groove into the bladder, the edge of the knife being held to the operator's right side, as in Buchanan's operation. The heel of the knife was then made to describe a portion of a circle corresponding to the external incisions, while the point, while being withdrawn, was moved little from the median plane. The operation was then complete. (Mr. H. Lee, p. 288.)

LITHOTRITY.—The size and form of the stone ought always to be determined before deciding which instrument to use. For this purpose the kind of sound to be used is by no means a matter of indifference. Many surgeons still use a sound having the form of an ordinary catheter. Such an instrument does not admit of much lateral movement in the cavity of the bladder, while rotation is impossible. Hence it often fails to detect a calculus. An exploring sound should have a short beak only, which can be moved in every direction with facility. By having the handle of this sound marked in eighths of an inch, upon which a small sliding ferrule is placed, we may with ease measure the exact size of the stone. The surgeon having struck the stone, moves the beak of the sound to the further end of it, and slightly taps its extremity. The sliding ferrule is then slipped down the stem till it touches the external meatus. He then slowly draws out the sound until it reaches the nearer end of the stone, and observes how much of the stem has issued from the urethral meatus, during the progress of the beak over the stone. The fact of the existence of renal disease is no bar to the performance of lithotrity. (Sir H. Thompson, p. 278.)

All the dangers and difficulties occurring to the patient in lithotrity may be broadly referred to one cause, viz., mechanical injury to the bladder and urethra. The sources of this injury are two: the stone itself, and the instrument employed to remove it. It is well to use a large flexible bougie on two or three occasions before determining on crushing the stone, in order to accustom the urethra to the passage of an instrument. If fever is easily aroused by so simple a procedure, the question of performing lithotrity must be carefully considered. At the first sitting the stone should be seized not more than twice (with the fenestrated instrument only when large) and merely broken into several pieces. In order that the fragments may become a little water-worn, and their edges less sharp, the patient should be kept in bed and admonished only to pass water for the next day or two when lying upon his back. The lithotrite should rarely remain at any sitting more than two minutes in the bladder. One minute usually suffices for three or four efficient crushings. Of course this is only possible with an instrument the construction of which admits of instantaneous change from screw to sliding movements, and *vice versa*; and for successful lithotrity this is absolutely necessary. We should diminish as much as possible the use of all instruments not absolutely necessary. For by far the greater number of cases a simple flat-bladed lithotrite is sufficient. Few matters are of more importance than the removal of the last fragment. There is sometimes uncertainty whether the symptoms are owing to a small fragment remaining, or to a slightly inflamed condition of the bladder. The best plan is to carefully explore once or twice with the sound, and the small beaked lithotrite, the blades of which are easily reversed and can be made to traverse the floor of the bladder. If nothing is discovered let the patient have a week or two's freedom from instruments, and during that time he should on two or three occasions take a rough drive on an omnibus or other jolting vehicle. Nothing more certainly increases the irritation if it is due to a fragment, while the effect will probably be very slight if none is present. (Sir H. Thompson, p. 280.)

STONE IN THE BLADDER IN THE ADULT.—Sir Henry Thompson gives a resumé of 100 cases of stone in the bladder, 84 of which were operated on by lithotrity and 16 by lithotomy. The mean age of the lithotrity cases was sixty two and a-half and yet the mortality was only five per-cent. of the whole.

He says, however, that lithotrity can be only thus successful when it is performed on a definite system, in accordance with certain practical rules which experience has determined, and which can be laid down. By exercising an ordinary degree of vigilance for adult patients suffering from symptoms of urinary disorder, every case of calculus may be discovered in an early stage, may be successfully treated by lithotrity, and consequently the operation of cutting for stone may be rendered obsolete, and applicable only for some very exceptional example which has been developed as the result of extreme neglect or ignorance. (Sir H. Thompson, p. 275.)

Stricture near the Urethral Orifice. — Strictures about two inches from the orifice of the urethra cannot be treated by dilatation ; it is perfectly useless except early in the case. There is so much fibrous tissue and so little of the spongy texture in old strictures, that the only plan is to divide them by means of a urethrotome after dilating them, if necessary, so as to admit of the introduction of that instrument. Nearer the bulb dilatation alone is the proper plan of treatment. (Sir H. Thompson, Medical Times and Gazette, July 27, 1867, p. 92.)

Varicocele.—The following is the plan of operating lately pursued at University College Hospital. A needle threaded with silver wire was passed between the enlarged vessels and the vas deferens, which were first separated a little from each other. The needle being withdrawn the loop of wire was left. The needle with a fresh loop of wire, was again introduced from the opposite side, but instead of passing behind the swollen vessels it was passed in front of them. It was again withdrawn and the loop left. A loop and two free ends of wire now projected at each side, and the ends being passed through their corresponding loops and drawn tight, the vessels were effectually strangulated. When ready for removal, a little pushing easily separated the wires. (Mr. Marshall, p. 292.)

FRACTURES, DISLOCATIONS, AND DISEASES OF BONES AND JOINTS.

Abscess.—*Antiseptic Treatment.*—In the treatment of abscess our great aim must be to prevent the introduction of those living particles of matter contained in the atmosphere, and which cause septic changes in the interior of the pyogenic cavity. A solution of one-part of crystallized carbolic acid in four parts of boiled linseed oil having been prepared, a piece of rag from four to six inches square is dipped in the oily mixture and laid upon the skin where the incision is to be made. The abscess may then be opened beneath the raised lower edge of the limb, which must be allowed to drop immediately the knife is withdrawn. The lint forms an antiseptic curtain beneath which the pus flows out into a vessel placed to receive it. If necessary a piece of lint soaked in the antiseptic oil may be introduced into the incision to check hemorrhage or to prevent primary adhesion, which is otherwise very apt to occur. This plan however, requires modification during the remainder of the treatment. We require an antiseptic dressing which will effectually prevent the decomposition of the stream of pus continually flowing from the wound. For this purpose a little of the above mentioned solution of carbolic acid in linseed oil should be mixed up with common whitening (carbonate of lime) to the consistence of a firm paste. This must be spread upon a piece of sheet block tin about six inches square ; or common tin-foil will answer equally well if strengthened by adhesive plaster. This should be applied over the abscess and retained in position by adhesive plaster. The dressing requires changing every twenty-four hours. The result of this treatment is that abscesses do not secrete any more pus after their original contents have been evacuated, the discharge being merely serum. (Prof. Lister, p. 151.)

Antiseptic Treatment of Wounds and Abscesses.—The necessity for an antiseptic treatment of wounds and abscesses is shown by the fact that, if iodine is put within them it is absorbed so rapidly that it is detected in the urine and saliva in a few minutes. If iodine is absorbed, why not the natural contents of the abscess ? (M. Demarquay, p. 207.)

ADHESION OF CICATRICES TO BONE.—It occasionally happens that great pain is felt in a cicatrix following amputation or other operation on the leg. This pain, however, is not confined to the cicatrix, but is referred to the whole of the surrounding parts, and it will not be discovered that the adherent portion of cicatrix is the seat of the pain unless the part is carefully examined. The proper plan is to divide the adhesion between the cicatrix and the periosteum subcutaneously, and then, by daily moving the skin over the bone, to prevent their reunion. One case is related in which a conical stump was forming apparently owing to the constant wearing pain from this cause, and the removal of which, by the treatment recommended, was followed by proper deposition of fat in the stump, as in other parts of the body. (Mr. H. Hancock, p. 196.)

Bony Anchylosis.—To ascertain whether anchylosis of a joint is bony or not, attempt to straighten the limb, and there will be no contractions in the muscles around the joint if the parts are immoveably fixed, whereas, if the anchylosis is only fibrous the surrounding tendons are felt to spring from their sheaths. (Mr. H. Coote, p. 201.)

Diseased Bones and Joints.—In the lower classes the treatment of diseased joints by rest is in most cases a failure, disease spreading from bone to bone in the smaller articulations, and some organic affection attacking the liver or some other internal organ before the consolidation of a large joint can be effected. The author states that he has met with great success by cutting down at once upon the diseased bone, even at the commencement of the affection, and after piercing the compact tissue by means of a trochar applying potassa fusa c. calce freely to the diseased bone, and along the track of the trochar. This treatment is most commonly called for in cases of congestive inflammation of the head of a long bone (which would terminate in disease of the joint.) In less acute cases a small eschar may be first made, the centre of which being incised, the caustic can be introduced, and by combining its action with the knife, the tunnel be carried deeper, from day to day, in a gradual manner. The same treatment is to be recommended in the advanced period of the case when caries is established, or when a whole joint is converted into a foul suppurating cavity, but it is not to be advised in the intermediate period, when the head of the bone is the seat of a diffused suppuration. The thing required in these cases is to put an end to the imprisonment of the diseased bone. (Dr. F. Kirkpatrick, p. 203.)

Dislocation of the Lower Jaw.—It is necessary for the reduction of this dislocation that the pterygoids, masseters, and temporals should be relaxed, as being the chief or opposing forces in its reduction. During the voluntary act of depressing the lower jaw these muscles are relaxed, therefore, just as you press downwards with the thumbs placed on the crowns of the posterior molars cause the patient to open the mouth a little. By this means the slightest pressure will be sufficient to disengage the necks of the condyles from the transverse roots of the zygomas, and will permit the temporals and masseters to at once reduce the dislocation by the reinduction of their normal action. (Mr. D. Kelly, p. 211.)

Dislocation of the Hip-Joint.—Supposing the dislocation to be one of the left side and upon the dorsum ilii, the following is the mode of procedure. Place the patient on a mattress laid on the floor, and put him completely under the influence of chloroform until the muscular system is thoroughly relaxed. The pelvis being steadied, seize the left ankle with the right hand, at the same time grasping the corresponding knee with the left hand. Flex the leg to its complete extent upon the thigh, and the thigh upon the abdomen. With the limb in this position the knee and foot must be made to take a circular sweep outwards as far as they will go, and then the whole limb must be suddenly extended. This movement causes the head of the bone to slip into the acetabulum at once with a distinct jerk. In some of the dislocations of the hip-joint the direction of the circular sweep should be made inwards, and a little rotation of the limb often assists the head of the bone into its place. (Mr. T. Annandale, p. 209.)

Excision of the Ankle-Joint.—Hitherto, in cases of disease of the ankle-joint incurable except by removal, the whole foot has been removed either by amputation at the ankle or by Syme's or Pirogoff's operations. There is no reason why the ankle-joint should not be excised as is done in similar disease of the knee or elbow, and so the foot be preserved. In Syme's operation, independently of the entire loss of the foot, there is danger of sloughing of the flaps, of bagging of matter, and in both Syme's and Pirogoff's, of inflammation, suppuration, and sloughing in the course of the divided tendons. In excision of the ankle-joint these dangers do not exist. Mr. Hancock, of Charing Cross Hospital, has now performed this operation five times—four times successfully, once unsuccessfully, the patient dying some six months after the operation from lung disease, the result of a life of dissipation. In no instance was there sloughing. There need not be a single artery or tendon divided. (Mr. H. Hancock, p. 185.)

One semilunar incision, extending across the front of the joint to behind the malleoli on either side and penetrating only to the fascia below, is the best way of commencing the operation. When this flap of skin is reflected we have plenty of room to see what we are about, and can avoid wounding the posterior and anterior tibial arteries, a point of great importance. If we meddle with an ankle-joint it is better to remove the entire joint, no matter how limited the disease may seem to be: when this plan has not been followed, the results have generally been very unsatisfactory. Where practicable the bones should always be removed by the saw, and the employment of the gouge avoided as much as possible. With the gouge we inflict an amount of violence, bruising, and crushing, which the parts already weakened by the preexisting mischief cannot repair, and thus the operation fails. (Mr. H. Hancock, p. 187.)

FRACTURES.—*Elastic Extension.*—The use of elastic extension is recommended in the treatment of fractures of the lower extremities. The means by which this is accomplished will best be understood by referring to the author's description of his splint. This plan seems to promise advantages in certain cases, especially when much spasmodic muscular contraction has to be overcome. In cases of fracture of the thigh with shortening, the limb will be found to have gained its proper length in two or three days. The extension may be increased or diminished according to circumstances. (Mr. H. Worthington, p. 178.)

Ununited Fractures.—Dr. Bigelow, of Harvard University, has been very successful in the treatment of ununited fracture. His plan of treatment is as follows: The false joint having been fairly exposed by a free incision in the part most free from important nerves and muscles, the ends must be turned out by flexing the joint and dividing any connecting material. The callus over-lying the periosteum at the tips, having been then incised by a crucial or other regular incision, should then be seized by strong-toothed forceps, and efforts made to tear it out of the rugous inequalities of the formerly inflamed bone. When the bone is reached, the periosteum must be stripped from both ends as far up as the intended section. The ends of the bones must now be sawn off. For the purpose of inserting the wire, holes must be bored in each extremity with a good bone drill, at a little more than half an inch from the end, and through one wall only. The wire must then be inserted from without inwards in one end, and inversely in the other, and the ends drawn accurately together, and the wire twisted and allowed to protrude at the external wound. The parts must be put up in properly made splints so as to preclude all motion. When union has taken place, the wire must be removed by division of the loop by cutting pliers, and be forcibly drawn out. It should generally be left in position several months. (p. 182.)

Fractures of the Femur.—We are all familiar with the advantages of suspension in cases of fracture of the leg, but not with the same principle of treatment in cases of fracture of the femur. Mr. Paget has almost done away with the use of the long splint in fractures of the thigh at St. Bartholomew's Hospital, and by a simple and effectual plan both suspends the limb and applies extension at the same time, thus getting rid of the irksomeness of the restraint and the objectionable perineal band incident to the older plan of treatment. The frame-work of the apparatus, or we may call it the splint itself, is two pieces of No. 2 iron wire passing down on either side of the thigh and leg, from the trochanter externally and the pubes internally to about six inches beyond the foot at each side of it. These are bent at the knee, and the nearer the fracture to that joint the smaller the angle should be. The limb however should in no case be much flexed. The leg and foot are pretty firmly bound to these side splints, but from two inches below the knee to the upper end of the iron wire, short pieces of bandage are applied on the under side of the limb only, running from one wire to the other, and forming a cradle for the limb carefully adapted to its shape. The upper surface of the thigh is therefore uncovered and open to inspection. By means of four hooks on the side wires or splints, two about the centres of the thigh pieces, and two about the centres of the lèg pieces, and by three small pullies, the whole limb is suspended by one piece of whipcord which is attached about seven feet above the bed to a point a little in advance of the foot of the patient. By this means there is constant extension kept up. If there is much tendency for the patient to slip down in bed, by placing small pieces of wood underneath the bottom legs we can slightly elevate the lower portion of the bed. This plan has been found very successful, and productive of great comfort to the patient. (St. Bartholomew's Hospital Reports, Vol. iii, p. 387.)

Fractures of the Neck of the Thigh Bone.—The late Mr. J. R. Hodgson, of Chesham, Bucks, was very successful in his treatment of cases of fracture of the neck of the thigh bone, as is attested by the specimens in Guy's Hospital Museum. His success may be attributed to his not disturbing the the impacted fragments. The treatment in these cases was the use of the double-inclined plane. The plane was made of pillows sewed together ; the foot was fixed to the sacking at the foot of the bed by a handkerchief folded round the ankle and over the instep, and tied to the sacking. The thigh and leg were fixed by shawls folded obliquely round the limb, above and below the ham, and secured to the under pillow of the inclined plane. This was maintained for fourteen or sixteen weeks. (Mr. S. Solly, p. 177.)

Fractures of the Leg.—Division of the Tendo Achillis.—Mr. Paget almost invariably divides the tendo achillis in cases of oblique fractures of the tibia and fibula, and in those near the ankle-joint with much displacement, and *always* in compound fractures. (St. Bartholomew's Hospital Reports, Vol. iii, p. 392.)

PATHOLOGY OF JOINT DISEASE.—There are but two structures in a joint which admit of primary inflammation—namely, the synovial membrane, and the articular extremity of the head of the bone. Ulceration of the articular cartilage is always a secondary affection, and depends upon a morbid condition of the cancellous tissue of the bone. In this inflammatory condition there is a layer of exquisitely sensitive granulations interposed between the painful and tender bone and the loosened cartilage. The joint is necessarily bent in order to relieve the pressure of the surfaces against each other. During the acute stage, no attempt at extension of the joint is admissible. (Mr. H. Coote, p. 201.)

Sleep after Operations.—Capacity for sleep after an operation is an excellent sign; such a patient will get through where another would not. (Mr. J. Paget, Lancet, Aug. 24, 1867, p. 220.)

Sub-Periosteal Resection of Bone.—In resection of bones, especially of the foot, it is a good plan to separate the periosteum along with the soft parts, 1st. Because the bone to be removed can in this way be thoroughly exposed; 2nd. Because the blood-vessels of the reflected periosteum, and flap or flaps of soft textures, are not in any way injured, except along the line of the incisions. In separating the periosteum from the bone, it is of great consequence to save as much as possible of its inner layer, as the experiments and researches of Ollier have shown that this is the chief bone-producing portion. (Mr. T. Annandale, p. 198.)

Sub-Periosteal Resection of the Os Calcis.—The following were the steps of an operation lately performed by Mr. Annandale, of Edinburgh, who has introduced this plan of resection of bones. An incision was first made along the outer aspect of the bone, extending to the articulation between the os calcis and cuboid in front. Two short incisions were also made at right angles to and at each end of the first incision, the anterior one of which corresponded to the direction of the joint between the os calcis and cuboid, and the posterior one to the course of the tendo achillis. The incisions were made quite down to the bone; and then, with one of Langenbeck's instruments the periosteum was scraped off and separated together with the flap of soft textures from the entire surface of the bone. By extending the anterior short incision a little the flap was easily turned downwards and inwards, and the bone was then detached and removed. The flap being re-adjusted, a gutta-percha splint was applied to the anterior aspect of the leg and foot, so as to keep the parts at rest. During the operation no artery required to be ligatured. (Mr. T. Annandale, p. 198)

Union of Wounds by the First Intention.—It having been proved conclusively that the non-union of incised wounds by the first intention is owing to the putrefactive changes in the blood and other discharges, it follows that by the prevention of such change by exclusion of atmospheric air we procure rapid healing of wounds and sores. A mixture of one part of carbolic acid with five of linseed oil by means of a piece of lint soaked in the liquid, is a good application to a wound. The success which is met with in the treatment of deep wounds by this plan renders it admissible to arrest arterial hemorrhage by torsion of the vessels. This mode of arresting hemorrhage is quite safe in the case of all arteries except the very largest. (Prof. Syme, p. 171.)

Wounds.—*Carbolic Acid.*—All the local inflammatory mischief and general febrile disturbance which follow severe injuries, are due to the irritating and poisoning influence of decomposing blood or sloughs. This decomposition depends upon the presence in the atmosphere of minute floating particles of organic matter, which owe their energy to their vitality. Carbolic acid has such a peculiarly destructive influence upon low forms of life, that by its application to a wound in a suitable manner, any decomposition is entirely prevented, and even the most dangerous wounds, as compound fractures, will heal without formation of pus. In treating any wound, first introduce the acid in full strength into all accessible recesses, by means of a piece of rag held in dressing forceps and dipped in the liquid. The compound formed by the action of the acid on the tissues need occasion no anxiety, as it is disposed of by absorption and organization. The next step is to lay on the wound, overwrapping it well on each side, a piece of lint soaked in a mixture of one part of carbolic acid and four of boiled linseed oil. This must be maintained permanently in contact with the wound, and never raised when the dressing above it (to be described) is changed. A paste made of common whitening and the carbolic acid and linseed oil, and of the consistence of putty, must be rolled out between two pieces of thin calico to a layer a quarter of an inch thick, and laid on the wound over the lint. This dressing must be changed daily so long as any discharge continues. It serves as a reservoir of the carbolic acid, and constantly maintains the subjacent lint in an antiseptic condition. It is a good plan to cover the dressings with a piece of thin sheet lead, or tinfoil strengthened with adhesive plaster, in order to prevent rapid evaporation of the acid. The results obtained are frequently surprising, serious wounds healing without the formation of any pus, and compound fractures uniting and the wound healing without any unfavourable symptoms whatever. (Prof. J. Lister, p. 156.)

Dr. Wolfe, of Aberdeen, whose experiments and published article on the subject of carbolic acid preceded those of Professor Lister, does not believe in the pan-spermatism hypothesis of M. Pasteur. He considers that the action of carbolic acid is due entirely to its antiseptic properties, that is, of preserving organizable matter from passing into a putrescent condition. (Dr. J. R. Wolfe, p. 165.)

Chloride of Zinc.—Chloride of zinc is constantly applied to recent wounds at King's College Hospital. It is kept in solution of moderate strength, and all the deep parts of a wound as well as the more superficial are well mopped out with it. This plan was first suggested by Mr. De Morgan, and especially recommended by him to be employed after removal of cancerous tumours, in order to thoroughly destroy any germs of the disease remaining in the wound. It is found to prevent much suppuration, and promote early healing of the parts. (Mr. De Morgan, Mr. J. Wood, p. 169.)

Sulphurous Acid as Applied to Wounds and Sores.—Sulphurous acid is superior in efficiency to carbolic acid, and is entirely free from the objection applicable to the latter, viz., of being a powerful irritant and having a disgusting smell. A case is related by Dr. Dewar, of Kirkaldy, in which the raw surface left after the amputation of the breast was carefully sponged with sulphurous acid, and the edges adjusted by four silver sutures. A piece of lint soaked in the liquid was laid over the wound, and this covered with gutta percha, the dressing being changed every six hours. There was never any pain in the wound, union of which was complete in twelve hours. (Dr. J. Dewar, p. 170.)

AFFECTIONS OF THE SKIN, ETC.

CARBUNCLE.—*Permanganate of Potash.*—In cases of carbuncle, great benefit will be found to result from dressing the swelling with lint saturated with a strong solution of permanganate of potash (ʒss. to f. ℥j.), and covered with oiled silk. A free incision is necessary first. (Dr. T. L. Leavitt, p. 300.)

ECZEMA.—*Iodide of Lead.*—Iodide of lead is a remedy of great value in eczema. It should be applied in the form of an ointment, twelve grains to the ounce, with one drachm of glycerine, and forty minims of chloroform, to relieve the itching. Another formula is the following: Iodide of lead, twenty grains; simple ointment, seven drachms; glycerine one drachm. The ointment of iodide of lead of the present pharmacopœia is too strong for cases of chronic eczema or psoriasis; it contains sixty-two grains to the ounce; whereas, from a fifth to a fourth of that quantity is sufficient, and more useful than the pharmacopœial strength. The use of constitutional treatment must be combined with this. (Dr. W. T. Belcher, p. 293.)

FAVUS AND TINEA CIRCINATA.—*Identity of the Fungi of.*—The opinion that the fungi of these diseases are identical is that of Dr. Tilbury Fox. It is confirmed strongly by some observations of Mr. John M. Purser, of Dublin. A cat having a patch of apparently favus on the paw, communicated tinea circinata to a female. The fungus of the cat was achorion, that of the woman trichophyton. (p. 295.)

HYPERTROPHIED CERVICAL GLANDS.—*Injection of Iodine.*—Dr. Marston, of Devizes, relates a case of hypertrophied cervical gland, the integuments covering which had ulcerated, leaving the diseased growth projecting, and of course of most unsightly appearance. By means of a Wood's subcutaneous injection syringe, he injected tincture of iodine into the centre of the tumour until it exuded from all parts of its surface. Neither pain nor inflammation followed, and in a week the tumour had diminished to nearly half its original size. The injection was repeated, and after the third operation the growth entirely disappeared, and the wound healed, leaving an almost imperceptible scar. This treatment seems deserving of trial in that numerous class of cases in which the skin remains unbroken, and which yet resist ordinary treatment. (Medical Times and Gazette, July 27, 1867, p. 87.)

SCABIES.—*Sulphuret of Calcium.*—The following is the best way of using the sulphuret of calcium. Quick lime, one pound; spring water, a sufficiency; sulphur, two pounds; water, twenty pounds. Boil to twelve pounds. The patient is placed in a tepid bath for half an hour; then, on leaving it, all the parts affected are rubbed with a piece of flannel dipped in this solution. The patient is then placed in a second tepid bath for another half hour. The same measures are repeated next day. (M. Vleminckx, p. 302.)

TUMOURS.—*Electricity.*—If electricity or galvanism is to be useful in the dispersion of tumours, it must not be applied with the object of decomposing them into their elements, but of modifying their nutrition. For this purpose a simple voltaic pile composed of from twelve to sixteen pairs of zinc and copper plates, one and a half inch square, with intervening pieces of felt, is sufficient. This battery can be rendered sufficiently active by immersion for a moment in salt and water. It is necessary that the negative pole should be placed over the tumour, never the positive, and the enlargement must be covered with a plate of copper exactly fitting it. The positive pole may be connected in a similar manner with some neighbouring portion of the body. A case is related in which a gland enlarged from secondary cancer to the size of a walnut was reduced "level with the surface" of the neck. Other cases equally encouraging are related. (Mr. M. H. Collis, Medical Press and Circular, July 24, 1867, p. 80.)

VASCULAR GROWTHS.—*Electricity.*—Vascular growths, such as nævus, papillary growths, and molluscum, may be destroyed by passing through them an electric current for a length of time varying with the size of the growth and the strength of the current. The negative pole (that connected with the zinc end of the battery) is the one to be applied to the growth, whilst the positive pole may be applied to some neighbouring part. In a case of nævus a needle was passed into the right half of the tumour and connected with the negative pole of ten cells of the battery, the current was then allowed to pass for two minutes, after which the needle was withdrawn. The right half of the tumour appeared shrunk and shrivelled up. Subsequently the other half was similarly treated, and the whole tumour was completely obliterated. (Dr. Althaus, British Medical Journal, May 11, 1867, p. 538.)

VENEREAL AFFECTIONS.

GLEET.—*Oil of Sandal Wood.*—Nineteen cases of gleet were treated with oil of sandal-wood. Thirteen cases received marked benefit, six none, and in four of these unpleasant symptoms, such as nausea, were produced by the smallest dose. Some stomachs tolerate oil of sandal wood which cannot bear copaiba. The average dose given was fifteen drops three times a-day, with a little liq. potassæ in peppermint water. (Mr. B. Hill, p. 304.)

GONORRHŒA.—*Starch Injections.*—Finely powdered starch mixed with lukewarm water, so as to obtain a fluid of the substance of cream, but thin enough to allow of injection, forms a most successful injection in cases of gonorrhœa, especially after the inflammatory stage is over. (M. Luc, p. 305.)

SYPHILITIC ULCERATION OF THE THROAT.—*Sulphurous Acid Spray.*—A spray of sulphurous acid injected upon the throat will give great relief in cases of syphilitic ulceration of that part. In a case in which it was so employed at the Royal

Free Hospital, the smell and bad taste which had been a source of great annoyance ceased immediately, and the unhealthy aspect of the sore gave place quickly to signs of healing. (Dr. Murchison, p. 305.)

Syphilization.—Syphilization has been placed well and impartially on trial at the Lock Hospital in London, and the conclusion come to is that it is not a plan of treatment which should be recommended for adoption. Even admitting that it has the advantages claimed for it by its advocates, (which it has not), the surgeons of the above hospital are of opinion that these are quite insufficient to compensate for the loss of time, personal discomfort, and indelible traces which it entails upon the patient. (Medical Times and Gazette, July 27, 1867, p. 105.)

Venereal Disease.—There is but one venereal virus, the action of which is usually chronic, and always ulcerative. The character of the sore depends upon the tissue upon which it is situated. The Venereal Commission appointed by the Admiralty has come to a different conclusion to the above, which is the opinion of Mr. Holmes Coote. (p. 302.)

AFFECTIONS OF THE EYE AND EAR.

Artificial Pupil.—The question often arises as to whether an artificial pupil should be made in one eye while the other is sound. This may be answered decidedly in the negative, except the pupil can be made in the centre of the iris, the lens being present or absent. If otherwise, there will be confusion of sight, double vision, or squint. It is singular, however, and unaccountable that a dissimilarity in the positions of the pupil is not invariably followed by this disturbance of vision. Supposing however that a central pupil can be made, the lens is nearly always absent, and minute sight must consequently always depend on the use of glasses of a high power, commonly called cataract glasses. The adjusting power of the eye is gone, and for seeing at different distances glasses of different foci are required. But, practically, an eye cannot be fitted with glasses and brought up to a state to match the other. Therefore arises this important question, on which the whole matter hinges: Will this kind of sight be really of material advantage? The answer is, decidedly, Yes. We should only be deterred from operating by the probability of the eye being too much damaged to afford that amount of sight known as "useful." (Mr. H. Walton, p. 319.)

DIVERGENT SQUINT.—The author operates for divergent squint, not by dividing the insertion of the external rectus, but by bringing forward the insertion of the internal rectus. The insertion of the latter muscle being reached in the usual way, a hook carrying a ligature is passed under it, and the ligature tightened on the end of the muscle. The succeeding steps are—a vertical incision in conjunctiva; traction on and loosening of the muscle by means of the ligature; passage of two ligatures through the muscle perpendicular to its plane, and situated at a variable distance from the ligature according to the amount of previous divergence; removal of the first ligature by cutting off the end of the muscle; fixation of the shortened muscle, by passing the ligatures one above and one below the cornea, to a point corresponding to the insertion of the superior rectus and between the conjunctiva and sclerotic. (Dr. C. R. Agnew, p. 327.)

MEMBRANA TYMPANI.—*New Mode of Examining.*—The membrana tympani may be easily illuminated and examined by means of a biconvex lens of $3\frac{1}{2}$ inches focal length, which must be held at such a distance that a cone of light is directed on the surface of the membrane. This has the effect of strongly illuminating the membrane itself, and at the same time keeping the surrounding parts in comparative darkness. For this purpose the lamp or other artificial light should be four feet from the head of the patient, and behind the head of the observer. When the meatus is sufficiently large in calibre, the speculum can of course be dispensed with. (Mr W. S. Watson, p. 333.)

OPACITY OF THE CORNEA.—Drop a little sulphate of soda in fine powder into the eye of the patient. It has the effect of causing absorption of the opaque material. This plan of treatment was suggested by the fact that sulphate of soda has the property of retaining the fibrine of blood in solution. (M. De Luca, p. 329.)

SOFT CATARACT.—Mr. Greenway, of Plymouth, who has had considerable experience in the extraction of soft cataract by suction, describes a suction curette which he considers an improvement on that generally employed. It is important that within the mouthpiece there should be a valve made of extremely thin india-rubber (oiled-silk will not answer), which is opened with the slightest amount of suction and is self-acting in closing. This prevents an accidental back current of air, a very important point when the eye is the part being operated on. It also serves the purpose of a maintaining power for holding whatever has been sucked into, or laid hold of by the canula. (Mr. H. Greenway, p. 317.)

MIDWIFERY, AND THE DISEASES OF WOMEN, ETC.

Cancer of Breast.—*The Acetic Acid Cure.*—A case of cancer of the breast is related in which Dr. Broadbent's acetic acid treatment was tried. The tumour was hard and rather deeply seated, and rapidly increasing in size. The acid employed was of the strength of one in four. It was most energetically used both by means of a Wood's syringe, and a small trochar. In three weeks' time, the report is "The disease spread rapidly, soon involving the whole breast, and ulcerating through the skin at one or two points of it, so as to leave me much too little skin to cover the wound, when, after the failure of the injection I felt compelled to remove the tumour." The wound did not heal satisfactorily, a small ulcerated surface being left, and this rapidly relapsing into a diseased state. A dressing of lint soaked in dilute acetic acid was applied constantly, but the disease spread rapidly till the whole cicatrix was involved in it. The patient ultimately sank with cancerous disease of the liver and intestines. (Dr. M'Gregor, p. 29.)

Cephalotribe.—The cephalotribe should be used in all cases of of embryotomy in preference to all crotchets, hooks, and craniotomy-forceps. The use of it would reduce the dangers of embryotomy to a minimum, and allow its performance in cases where delivery could not otherwise be accomplished except by Cæsarian section. In one instance in which it was used there was but one inch and three quarters in the antero-posterior diameter of the pelvis, and yet the mother was delivered safely and made a good recovery. For the information of those who have not seen one of these instruments, we may describe it as a most powerful two-bladed forceps, the blades nearly two inches in width, solid, and toothed. By means of a screw in the handle, they may be made to crush bone, or simply to act as prehensile forceps for the purpose of extraction. Simpson's cephalotribe has a pelvic curve. Dr. Kidd, who has had much experience in the use of this instrument, has adopted a straight blade. He believes this has the advantage of permitting the head to be rotated in a smaller space than with a curved blade. This is of great importance in removing the crushed head of the child. It is also easier to guide a straight instrument than a curved one. (Dr. J. B. Hicks, Dr. G. H. Kidd, pp. 368, 370.)

Sir James Simpson prefers a curved cephalotribe, and states that the advantage is that it will not slip where a straight pair of blades will. The cephalotribe with teeth for fixing the scalp of the child, he regards as a great improvement. (p. 369.)

CHLOROFORM IN LABOUR.—In a small work "On Safe Delivery from the Pains of Labour," the author recommends, for the purpose of procuring "painless parturition with full mental consciousness," one part of eau-de-Cologne with two of chloroform. (Mr. I. Brown, p. 343.)

EMBOLISM AND THROMBOSIS.—*Death from Pulmonary Obstruction following Labour.*—A case at King's College Hospital is related in which, on the fifth day after labour, sudden collapse came on, with urgent dyspnœa. Death rapidly ensued, and the pulmonary arteries were found to contain dark-red, soft, unadherent clots. Both cavities of the heart were empty. No clots were found in the uterine, ovarian, or femoral veins. The uterus itself presented nothing unusual. When death occurs from primary coagulation of the blood in the vessels of the lungs it is generally within a moderate time after delivery. There are other cases in which the fatal issue is preceded by well marked symptoms of phlegmasia dolens, or crural phlebitis, or coagulation of the blood in some of the veins of the lower extremities. No sooner is the fibrin deposited than it begins to undergo certain changes consisting chiefly in a retrograde metamorphosis, which is generally either an amylaceous or fatty degeneration. The result is that the coagulum becomes softened ; and thus some accidental cause gives rise to the detachment of a portion, which being carried to the right side of the heart and thence to the pulmonary artery, at once produces the phenomena of embolism. Death from this cause rarely occurs before three weeks from the time of delivery. When thrombosis occurs after labour, in the large majority of instances the patient has been in a very feeble and exhausted state from flooding, severe labour, or other causes. (Dr. W. S. Playfair, p. 336.)

MIDWIFERY FORCEPS.—The rules laid down by most writers on the use of the midwifery forceps, as Denman, Robert Lee, Churchill, Meigs, and Ramsbotham, are far too restricted. It is not necessary to wait six, four, or even one hour, so long as we are satisfied that the labour if left to nature will be tedious and protracted : a point which it must be admitted requires much judgment and experience to determine. The rule should be discarded that before the forceps are applied the ear should be felt, neither is it always necessary to wait until the os uteri is fully dilated ; for with ordinary care the blades may be passed beyond the os uteri without inflicting the least injury, and with properly constructed forceps the os uteri is not stretched until the head itself is brought down to assist in the dilatation in the natural way. Even where the fœtus is dead extraction by the forceps is preferable to craniotomy, as being safer to the mother and less repulsive to her feelings and those of her friends. (Mr. A. B. Steele, p. 344.

It is generally supposed that the hold of the forceps upon the head depends upon the grasp of the accoucheur on the handles of the instrument. This is not the case. The hold is nearly as firm when the handles do not cross at all. Mattei, of Paris, has made an instrument whose parallel blades are set in a cross-bar of wood to serve for traction. In the case of the short handled forceps there is little or no compressive power. The hold is really due to the curvature of the blades, which fit more or less accurately upon the globular head, and the compression of the bows of the blades against the soft parts of the mother, supported by the bony ring of the pelvis. The greater the curve of the forceps blades and the wider the fenestræ the better they hold. Those blades which, when in apposition, approach parellelism are more liable to slip. In the French and Continental forceps the blades when in apposition form nearly a circle, and the points approach nearer than in the English patterns. (Dr. R. Barnes, p. 348.)

Application of the Long Forceps.—There is this difference between the long and short midwifery forceps, that the former is invariably applied in relation to the pelvic diameters whatever the position of the head, whereas the latter is applied in relation to the fœtal head. When the long forceps are used the position of the head may be practically disregarded. The pelvic curve of the blades indicates that these must be adapted to the curve of the sacrum in order to reach the brim. They must therefore be passed as nearly as may be in the transverse diameter of the pelvis. One blade will be in each ilium, and the head, whatever its position in relation to the pelvic diameter, will be grasped between them. The universal force of this rule much simplifies and facilitates the use of the instrument. (Dr. Barnes, p. 352.)

If the blades lock when applied, it may generally be taken as evidence that they are properly adjusted to the head. If they do not, either they are improperly applied, or the pelvis is not adapted to their use. If the blades cannot be got to lock, that is, the second blade to pass exactly opposite to the first, and both in the transverse diameter of the pelvis, the attempt must be given up and the hand passed into the pelvis, if necessary, to explore its dimensions and form carefully, and to determine between turning and craniotomy. (Dr. Barnes, p. 354.)

The following manœuvre is occasionally serviceable, and in certain cases may enable us to deliver without resorting to the forceps or lever. Pass a finger into the rectum, so as to get a point of pressure upon the forehead. In this way it is sometimes possible to bring the face downwards, to start the extension movement, and thus to extricate the head delayed at the outlet. And if at the same time firm downward pressure be made upon the breech through the fundus, the force propagated through the spine will aid materially in giving the extension movement. (Dr. R. Barnes, p. 357.)

Dr. Beatty's Midwifery Forceps.—These forceps were first described by the inventor twenty-five years ago, since which time he has constantly employed them and used no other instruments. The old short forceps have been totally abandoned in Dublin in their favour. They are designed as intermediate between the long clumsy French forceps and the old short ones of Smellie. Their entire length is twelve and a half inches; of the blade to the lock, eight inches. It will be seen that the blades, which have no lateral curve, are unusually long, they are also at the greatest breadth only an inch and three-eighths. The curve of the blades is not great, in order to facilitate their introduction. In using this instrument, the point should never be urged in the direction of the long axis of the blade. Such a proceeding is calculated to inflict injury on the mother. The blade should first be laid with its concave surface flat on the child's head, and the handle projecting between the thighs of the mother. It must then be gradually introduced by a series of lateral sweeping movements into its proper place, no force whatever being used. (Dr. T. E. Beatty, p. 359.)

The forceps may be used with all the adroitness possible, and all the force allowable as extractors, but without success in many cases of occipito-posterior position of the fœtal head; whereas, a mere tithe of the same force will answer if the head be rotated at the same time, and the forehead be turned backwards and to the left, and the occiput forwards and to the right. If this manœuvre be attempted with curved forceps, on its accomplishment we shall have the points projecting backwards, and liable to inflict great injury on the mother. (Sir J. Y. Simpson, p. 367.)

From the preceding observation of Sir James Simpson, it will be seen that Dr. Beatty's forceps are applicable in an important class of cases, in which the curved instrument of Dr. Barnes would not answer the purpose. (Dr. H. E. Eastlake, p. 367.)

New Needle for Fissure Operations.—Dr. P. H. Maclaren, of Lasswade, Edinburgh, recommends a needle for facilitating the introduction of metallic sutures in operations for the cure of vesico and recto-vaginal fistulæ, and congenital palatine fissure. This needle is 3½ inches long, perforated throughout, and fixed in a handle. Its essential peculiarity is that its free extremity for an inch and a half describes the half of a wide spiral. It may be obtained from Mr. Young, of Edinburgh. (Glasgow Medical Journal, July 1867, p. 102.)

Speculum.—*The Duck-bill.*—The duck-bill speculum is preferable to all others for examinations and operations on the vagina and uterus. Sunlight is not the most suitable kind of light; and it is preferable that the light should enter the room from one window at the back of the operator rather than from different quarters. A table about four feet long covered with a blanket, is preferable to a bed or couch for the patient to lie upon. The position is on the left side, body diagonally to the table, so that the buttock rests on the left of the angle next to the operator and the window. the thighs being bent at right angles to the trunk. (Dr. J. M. Sims, p. 385.)

Sponge Tents.—*Carbolised.*—Sponge tents may be rendered incapable of putrefaction by introducing into the core of the tent several threads of cotton wick steeped in carbolic acid; and after the sponge is rolled into its proper shape, by immersing it in cocoa butter to which a certain quantity of glacial carbolic acid is added. The disinfectant properties of this agent completely protect the tents, and they are withdrawn in an inodorous state even after a stay of eighteen or twenty hours in the cervical canal. (Mr. R. Ellis, p. 386.)

Urethritis in the Female.—The two principal causes of this affection are gonorrhœa and parturition. After the acute stage has passed off we can only rely upon local remedies. The most reliable are the following—1. Nitrate of silver. This should be held in a small caustic holder on the end of a stilet and sliding easily within a small silver tube, from which the caustic can only be made to project a short distance. The nitrate should be sheathed in the tube when passed to the vesical end of the urethra, and then being projected beyond the silver tube, the whole should be withdrawn. By this means the mucous membrane is lightly touched throughout its whole extent. 2. Dip a moderate sized gum elastic tube in gum water and then in powdered tannic acid. After it has dried it should be passed gently through the fingers to remove any roughness, and when wanted for use, is just passed gently into the urethra, and allowed to remain about fifteen minutes. 3. A stick of dried sulphate of zinc may be passed up the passage and immediately withdrawn. Any of the above plans of treatment is effectual. The mode of using the nitrate of silver is also suitable for the cervix uteri in chronic inflammation of that part. (Dr. J. B. Hicks, p. 392.)

UTERUS.—*Fixation of by a Tenaculum during Operations on it.*—Whenever anything is introduced into the uterus, be it a sound or a tent, a syringe or a knife, the cervix should be fixed with a tenaculum, after introducing the duck-bill speculum. By this means steadiness is ensured, and, when necessary, the neck of the womb may be drawn down into the vagina, and so directed that the surgeon can at once see into it. (Dr. J. M. Sims, p. 385.)

VASCULAR GROWTHS AT THE MEATUS URINARIUS —These are of two kinds, polypoid and sessile. The first should be removed by means of a wire snare, and the base touched with nitric acid. The second should be destroyed with nitric acid. For this purpose nothing is more convenient than to take a common lucifer match, cut off the inflammable portion and round the tip, which should then be dipped in nitric acid, the superfluous liquid wiped off, and the saturated wood applied to the part to be destroyed. No caustic acts so well as this. (Dr. J. B. Hicks, p. 397.)

MISCELLANEA.

BICHLORIDE OF METHYLENE AS AN ANÆSTHETIC.—Dr. Richardson proposes the bichloride of methylene as a substitute for chloroform. It has one physical property of great importance and value, viz., that it volatilizes at a temperature lower than that of the body. It is hence thrown off from the system with great ease, and the patient recovers from the effects of the vapour almost instantly. At the same time the sleep produced by the bichloride is more prolonged than that produced by either chloroform or tetrachloride of carbon, and is not preceded by any excitement as in the former, nor followed by any drowsiness or dulness as in the latter. On animals, the bichloride acts more evenly on the respiration and circulation than any other anæsthetic. When the breathing is quickened the pulse also is quickened in proportion, and when the breathing is slow and tranquil the pulse also is the same. This is a good point, because there is no condition more perilous than the disturbed balance of the respiratory and circulatory systems. The bichloride dissolves in water, and the solution possesses intoxicating properties. This solution may probably be found to be useful as a soothing medicine, or anodyne. (Dr. B. W. Richardson, p. 391.)

BLOW-PIPE GAS CAUTERY.—This is an instrument by means of which the intense heat of the blow-pipe flame is made available as a cauterising agent. The supply of gas is contained in an india-rubber ball filled from an ordinary gas-jet, and as the lighted gas is forced from this it is converted, by a current of air, which impinges upon the flame, into a sharply-pointed conical jet, possessing sufficient heat to fuse the ordinary metals. The whole apparatus is packed in a small box, and has the advantage over the galvanic cautery both in portability and cheapness. It is made by Mr. Baker, of Holborn, from the design of the inventor, Mr. Alex. Bruce. (see woodcut, p. 399.)

CASTS.—*An Easy Method of Taking.*—Paint the part of which it is desired to take the cast with melted paraffin by means of a large brush, and as each layer dries put on another until the paraffin is a quarter of an inch thick or more, according to the size of the cast. During the few minutes in which the paraffin remains plastic, a sharp pen-knife is to be run over the lines requisite for the removal of the matrix. A cold water cloth should then be placed over all for five minutes, when the pieces may be removed and reunited by a hot wire. Plaster is then to be run into the matrix in the usual way, and after it has set the paraffin is to be carefully broken off, and the cast dressed. (Mr. Lawson Tait, Medical Times and Gazette, Aug. 17, 1867, p. 168.)

CHLOROFORM.—On the whole we have no safer anæsthetic than chloroform, if it be administered with steadiness, boldness, and attention. Much of the mischief attending its use arises from handling it too cautiously, in a too fearful way, prolonging thus the alarm of the patient and chances of spasm of the glottis. The latest deaths have been from mixtures or "mixed vapours." (Dr. C. Kidd, Glasgow Medical Journal, May, 1867, p. 16.)

DISINFECTION BY CHLORINE.—Mr. Stone, Professor of Chemistry at the Manchester School of Medicine, has constructed an apparatus which will, continuously and with graduated flow, supply chlorine in such a way as to be available for use in the sick chamber or the wards of an hospital. As a substitute for disinfection by chloride of lime, it is infinitely preferable for obvious reasons. By Mr. Stone's arrangement, a mechanism is afforded by which the amount of chlorine generated can be exactly regulated. (Lancet, Aug. 31, 1867, p. 272.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

1.—ON TYPHOID FEVER.

By Dr. Jenner, Physician to University College Hospital.

Of all rapid and acute diseases, Dr. Jenner thinks that typhoid is the one which commits the greatest ravages among young people, and that, too, in a guise not very easily detected at all times, for the eruption is far from constant. But there is one clue, which may be often available when no others are. This, says Dr. Jenner, is enlargement of the spleen, which commonly occurs in this form of disease. A case in point fell under his notice not long ago. A boy fell ill of typhoid, but gradually recovered, and was sent on a visit to another part of the country. He did not, however, seem to thrive, and the question arose whether his complaint—for it practically amounted to that—was the result of dietetic irregularity, or was a distinct relapse of the fever. There were no spots, and there was no diarrhœa, but the enlarged spleen enabled Dr. Jenner to make a positive diagnosis of relapse of the fever. Of all the facts he has been able to make out in connexion with this important disease, Dr. Jenner himself looks upon two as being the most valuable—viz., the limitation of the abdominal complication to the glandular system, and the fact that if headache prevail throughout the disease, this is due to cerebral mischief, not to the disease itself. With regard to treatment, nobody now-a-days attempts to avert or to cure a fever, yet there are probably few diseases in which a skilful physician can do more for the benefit of his patient, and this in various ways. In the first place it is known that, provided we can keep a patient alive a certain time, the disorder will pass away; hence, by studying the tendencies to death, and by obviating these, the patient may be enabled to tide over this critical period, and so be restored to health. But it is not with these tendencies alone that the physician has to deal, for throughout the whole of the disease complications of various kinds are liable to spring up, and these also must be combated. At the bottom of the whole matter, however, Dr. Jenner thinks, there is the intense prostration produced by the fever, tending to cut off the patient tion may probably be found to be useful as a soothing medicine, or anodyne. (Dr. B. W. Richardson, p. 391.)

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2.—ON MIXED TYPES OF FEVER; IN RELATION TO THE QUESTION OF THE IDENTITY OR NON-IDENTITY OF THE TYPHUS AND TYPHOID POISONS.

By Dr. HENRY KENNEDY, A.B., Dublin.

[The question of the identity or dissimilarity of typhus and typhoid fevers seems in England to be quite decided in favour of the latter. Turn to any work lately published on the subject, or to the doctrines taught at the London hospitals, as shown in the published clinical lectures, and this will forcibly appear. Dr. Kennedy, however, believes the two forms of fever to be identical, both owing their origin to the same poison. He observes that the work of Huss on this subject has always been utterly ignored in this country. He saw fevers on a far larger scale than any British physician.]

Surely, in such a widespread disease as fever, no one is justified in asserting that what they have seen is what must have been seen by others. We know that even in the type of fever familiar to us as typhus, very great differences exist, and may be constantly seen when the disease attacks several members of the same family. In one, the head symptoms are all in the ascendant; in a second, the chest will be the part attacked; whilst in a third, it will be the stomach in the form known as gastric fever. Or, again, as regards the spots, the husband will present them and the wife not, or the parents will have them and the children not; or it may be the converse of this. In a family named Bright, of whom eight were in hospital at the same time, and who were sent in by Dr. Carte of the Royal Hospital, the children were all spotted, whilst the mother had none, though she had a very severe attack of fever. Again, in three sisters, all adults, who were recently in hospital, only one had the regular spots of the disease; in the other two anything of rash was most indistinct; one of these latter died. Further still, in the great epidemic of 1847-48, the fever was what is known, and had been described previously, as the relapsing fever; that is it was made up of two parts. There was a sharp attack of fever running on for five or seven days; then a lull of one, two, or three days; and again an onset of fever, usually much severer than the first, and, in very many cases, attended by spots. No one, I think, could have any doubt but that it was one and the same poison that caused the two attacks; and yet, in all the recent and standard works on fever, the relapsing type is described as if it were a totally different fever from typhus, and caused by a different poison. I cannot give in my adhesion to this opinion, for I have as strong a conviction as the nature of the subject admits that the poison of typhus generates not only the type of fever known as relapsing, but other types, such as nervous, gastric, cerebral, &c., as also fever, both without and with spots, and presenting all the variety which they are capable of exhibiting; and if this view of the typhus poison be not held, insuperable difficulties, as it appears to me, must arise when we come to consider analogous diseases to fever, as, for instance, scarlatina. Here every one must have seen the great variety—I might almost say contrasts —which this disease often presents in the same family and at the same time. Yet no one ever thought of setting down these differences to different poisons; and why it should be necessary as regards fever it is not easy to understand. I must leave this point to others to settle.

From the tenor of these remarks it will be understood what are the views I hold on the question more immediately under discussion. I believe that the typhus poison is capable of engendering the type of fever known as typhoid or enteric, and that this particular type must be due to some other cause rather than a specific poison. On the other hand, I hold that the two types can, in the great majority of instances, be distinguished, the one from the other. When I brought the subject first before the Royal Medico-Chirurgical Society of London, in 1860, one of my arguments consisted in the detail of a few cases which were directly opposed to the views of the London physicians. In a later paper, published in the Dublin Quarterly Journal, a still larger number of cases were given, and I cannot, I believe, do better now than by giving the briefest sketch of some which have come under my notice within the last two years. But, in truth, I may say the difficulty now consists in selecting the cases, they have become so numerous. So I shall take such as bear most directly on the disputed point.

Case 1.—McKeown, aged 17, having a fine skin, passed through a very severe attack of enteric fever. Every symptom was present, and during its progress the brain was much engaged, and the tongue and lips covered with sordes. He made a good though slow recovery.

Case 2.—His brother, aged 12, from same room, was admitted under a severe attack of typhus. He had the well-marked and copious rash of the disease, and his face was quite characteristic. He had a sharp attack of diarrhœa, calling for special treatment. My friend, Dr. Hudson, was kind enough to come and confirm my diagnosis of this case. It is but right to state there was an interval of a week between the admission of these two brothers.

These two cases have been given as affording an example of the two types of fever, each well marked, coming from the same room. Others, I know, have met similar examples, and Dr. Croly of Harcourt-street, has informed me of a very striking one.*

Case 3.—McCauley, aged 18, fine skin, was handed over to my care by my friend Dr. Moore. The patient laboured under fever, and had the spots of the enteric type very well marked on abdomen and sides of the thorax, but there was no other sign whatever of this kind of fever. His illness ran on for many days, the chest becoming engaged, and when he left hospital there were signs about him as if phthisis might supervene.

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* After the reading of the paper Dr. Croly detailed three cases of fever which occurred in the one room. The first was a case of enteric fever, the second a case of typhus, whilst the third was a mixture of the two types.

Cases 4 and 5 were of a similar character to the one just given—that is, with fever, the spots were those of the enteric type, but no other symptom of that kind of fever. As they were published, however, in the Lancet for December, 1864, I shall say nothing more of them here.

Case 6.—Podesta, an Italian, 14 years of age, and of a very fine skin, admitted into hospital in September, 1865. In the course of his fever he presented a very good example of the spots said to mark enteric fever. They were few in number, and appeared on the sides of the chest and abdomen. Neither in this case was there any other sign whatever of the enteric type of the disease.

Case 7.—Develin, a young man of 17, admitted into hospital during the present month, April, 1866. He had fever, but not of a severe kind, marked by the usual symptoms, and the tongue red and furred. When he was now six days ill the spots of enteric fever appeared on chest and abdomen, and in an unusually well-marked form. On the second day of their appearance this patient was seen by the Drs. Martin, from Berlin, who happened to be visiting the hospital. On the third day, however, the number of spots had greatly increased, and become more those of typhus, and finally the case, beginning with the spots of enteric fever, became one of irregular typhus.

Case 8.—Keegan, a man of 27, admitted March, 1865. He was labouring under heavy spotted fever. Some of the spots were large and dark, some were unusually well defined and red, and disappeared on pressure. The case, however, was one of regular typhus and the man made a good recovery.

Case 9.—Murphy, girl of 19, whilst passing through a severe attack of fever, with typhus spots, got a very sharp attack of diarrhœa, attended by tympany, and pain on pressing the ilio-cœcal region. Nothing checked this diarrhœa till special treatment was adopted.

Case 10.—A dumb girl, aged 24, sent into hospital from the South Union. She had bad fever, being all covered with a copious measly rash, whilst the tongue, face, and eyes were those which mark typhus. In the progress of the attack she got severe diarrhœa, attended by tympany, and distinct pain when pressure was made on ilio-cœcal region, and only here. This complication required specific treatment, and she got steadily but slowly well.

Case 11.—Dixon, man of 25, of a very fine skin, and thin, admitted into the Cork-street hospital, labouring under fever, and with a copious rash of typhus spots over him. His general aspect that of the same type of fever. As the disease went on he got severe diarrhœa, the discharges being a light yellow colour, and attended by distinct pain in right iliac region, and tympany. This man also required specific treatment, and the attack was one of unusual severity, marked by great distress and restlessness. His recovery, too, was much prolonged by the occurrence of several abscesses.

Case 12.—In February, 1865, Kelly, a man of 19, was admitted into hospital. He was evidently very ill; but the symptoms of typhus and enteric fever were so mixed up that I was quite unable to say to which type of the disease the case ought to be referred. He had a copious rash over the body, and his expression was that of a man in typhus. But he had also slight though marked tympany, distinct pain on pressure over the ilio-cœcal region, and a very severe diarrhœa, the discharges being of a light yellow colour. He made a very slow recovery. *This man's sister was in hospital at the same time. She had typhus.*

Case 13.—Woods, a man of 20, came in with a kind of spurious fever on him. He then went out for some days, but returned in a week with every sign of enteric fever on him except the rash. He had, however, spots on him, which to my surprise turned out to be variola in the discrete form. Whilst still in bed from this he seemed one day to get suddenly worse, and then typhus in a very severe form declared itself. During all this time he had sharp diarrhœa, and the discharges were those which I believe to be most characteristic of enteric fever, being of a light yellow colour. This patient's life was in the balance for many days, but he finally recovered.

Case 14.—Burn, a girl of 16, admitted in July, 1865. She then laboured under a severe attack of typhus, being well spotted. She was so far advanced as to be sent to the Convalescent House, when she again sickened, complaining of her head, and this again followed by great raving and high fever. When now a week ill, the spots of enteric fever made their appearance. These were unusually well marked, being few in number, and confined to the sides of the chest and abdomen; but there was no other symptom whatever of enteric fever, and they were looked for, I need scarcely say, with the greatest minuteness, nor did any such appear. At this stage of her illness the patient was seen by Dr. Murchison of London, who was visiting Dublin at the time, but who, I regret to say, I was not fortunate enough to meet.

Such is the series of cases which I wish to bring under the notice of the Association this evening. When added to those already given in the two former papers—and did time permit, I could have given other similar cases—they appear to me to afford the strongest proof the question is capable of eliciting, that we must consider the two types of fever known as typhus and enteric as the result of but one poison. If this be not the correct view to take of the matter, I confess myself quite unable to explain the cases of the mixed types detailed this evening; for it must have been observed, as each was given, how the symptoms of each type of fever were mixed up together. As there is not time, however, to go over each symptom in detail, I shall notice but one, on which most, if not all, who hold different views from my own, seemed to have placed the greatest weight of their argument. I mean the spots said to be characteristic of enteric fever. On this point, I think may say with certainty that these lenticular red spots, and few in number, have not the value which has been given them; for I have seen them now in many instances, and some have been given this evening where, whilst they existed, there was not another symptom of the ileum being engaged—at least I could make out no evidence of such a lesion, though looking specially for it. Here, then, were cases where the particular spots existed, but not the lesion of which they are said to be diagnostic. But, further still, I have given cases to-night where, with the enteric spots, there was also a typhus rash. As bearing on this particular point, I would just recall the case of the man Develin, where the enteric spots first appeared, then the typhus rash, and as this latter disappeared the enteric spots were again visible. If this be not a case in point, I know not what is; and I shall be glad to hear some explanation of this from any gentleman who differs from me. As regards the spots of typhus fever generally, I have got an impression that a good deal of misconception exists. I have heard some speak of the bright and the dark spots, as if there were a difference between them. On this point I can state with certainty that it is very common to see the two on the same individual, and at the same time. This may be seen on the body itself, but it is more common to have them dark on the body and a bright red on the arms. Again, the spots of enteric fever are described as recurring again and again, and this is quite true. But it does not seem to be so generally known that the same may be seen in typhus, for I have witnessed cases where a distinct second crop of eruption appeared; nor is the observation original, as I have read of it in one of the olden authors, though I cannot at this moment give his name. So also of the statement that petechiæ are never seen on the face. This is positively incorrect, as I have noted several cases where they were quite distinct. But these points are only mentioned here as bearing indirectly on the point under discussion. Still I think they are enough to show that any positive statements about the rash in fever must be received with caution, as the variety is truly very great. I cannot, however, pursue the subject further here.

In the course of these observations it has been stated that the enteric type of fever must be due to a something else rather than a particular poison; and if asked what that is, I would state my impression that it only occurs in persons of a peculiar constitution, most probably closely connected, if not identical, with the strumous. This idea I have stated before; but every year is increasing my conviction on the point, and if it should turn out to be correct, I need scarcely say how important it would be. I know not whether the idea has struck any one else, but it is not stated in any of the works on the subject that I have seen. My reasons for holding this view are the following:—The enteric fever is very constantly indeed met in persons of a fine skin, and I have now seen several instances where scars, evidently strumous, existed in the neck of persons who had this type of fever. Again, it is much more common under 30 years of age—that is, when the tendency to struma is known to be strongest. I am aware that this remark may be objected to, inasmuch as every type of fever is more frequent under 30; but what I would convey is this, that whilst typhus is common after 30, 40, and 50, enteric fever is exceedingly rare. I myself have not met it in any instance above 35, though it has, I know, been seen later; but, further still, every one is aware that in the course of enteric fever the lungs are very apt to become engaged. But in place of this affection passing off with the fever, as it does in typhus, it is by no means uncommon to meet cases where signs like phthisis declare themselves. The pulse keeps up, sweating occurs, and the cough is very troublesome and hard to relieve. I have said that such is common after enteric fever, and I have been forced to send several out of hospital in this state with the hope that change of air would benefit them, and in some I know that I heard of subsequently it had proved successful. That the idea I would put forward is not without some surer foundation than mere impression, I may cite the following instance:—

Case 15.—C., a girl of 16, was admitted into the Cork-street Hospital in January of the present year. She had a very fine skin, with light eyes and hair, and laboured under enteric fever in a very well-marked form. The diarrhœa proved most obstinate; but as the abdominal symptoms yielded the lungs got very much engaged from general bronchitis of the minute tubes, and for more than ten days the dyspnœa was of the most urgent character, the lips being quite livid and the distress very great. Though the urgency of this state lessened, the pulse still kept up, and the patient began to have regular sweats, and, finally, I was able to observe that the upper part of the right lung was becoming solidified. Nor did the disease stop here, for in a period of about seven weeks I was able to trace weekly the process of softening going on, till at last a cavity formed. In this state the patient left hospital, the physical signs in the top of the lung being those of a cavity, but the rest of the lungs being apparently quite sound, and as the patient's passage had been taken for America, it is just possible the predisposition to tubercle, which seemed so strong in this girl, may be averted, and she might yet live to old age.

Lastly, on this question of the connexion, or supposed connexion, between enteric fever and the strumous diathesis, I would just advert to the great similarity which obtains between the lesion found in the fever and that which so often exists in ordinary phthisis. For my own part, I must say that I have seen many specimens where I could not distinguish them, and I shall be glad to hear any gentleman express his opinion on the point.

The general question brought before the Association this evening is not, as some think, one of mere curiosity. It is of every importance that it should be settled. The diagnosis, prognosis, and treatment of the disease all hinge upon it. For if typhus be the specific fever which some think it, it is obvious that the treatment will differ from what it would do were the enteric lesion present at the same time, and the danger of allowing such a lesion to pursue its course unchecked would indeed be very great. On the other hand, those who hold with myself that the two types of fever may arise from the one poison and coexist, will always be on the look-out for such a complication, and will act accordingly. For myself, I believe I have often had to deal with such cases, and to alter or modify the treatment as the case required, and that this is not a mere belief I have reserved for this part of my remarks the details of the following cases, which have, however, been on a former occasion detailed.

Case 16.—A girl of 20 years of age was attacked with fever of a severe kind. Raving occurred and petechiæ very early, and these latter spread over the entire body. With these symptoms there was also severe diarrhœa and tympanitis. Matters went from bad to worse, and the patient died about the fourteenth day of the fever. On post-mortem examination the lower portion of the ileum was found extensively ulcerated, Peyer's patches being the parts engaged.

Case 17.—A boy of 14, who had already learned to drink, was attacked with fever. He had much stupor and moaning, both night and day, and he presented a copious petechial rash over the body. With this state he had also tympany and diarrhœa, and, finally, involuntary stools and death. On examination extensive ulceration in patches was found in the lower portion of the ileum. The brain presented the usual appearances found in cases of fever, but in a lesser degree than is common. I should say at the time this case occurred I was much surprised at the result of the post-mortem, for I then believed the enteric lesion could not exist with regular typhus, which the boy otherwise presented.

Case 18.—Hill, a girl of 18, fine skin, was admitted into hospital after being nine days ill of fever, which presented all the signs of the enteric type, including the spots, which appeared the day after admission. These did not, however, go through the usual course of such spots. They gradually increased in numbers, spreading to the chest, arms, and, finally, the face, and in this state many of them could not be distinguished from regular petechiæ, being large, dark, and ill-defined. My colleague, as he was then, Dr. Aquilla Smith, saw the patient at this period. By the fourteenth day of the fever all the signs of enteric fever seemed to have subsided, but there was no corresponding change in the state of the patient. Her nights became restless, she shortly lay on her back, sordes formed on the nostrils, lips, and tongue, and she got great tremor of the upper extremities—in fact, she presented all the signs of well-marked typhus, and died on the twenty-first day of her illness. Except in the lower portion of the ileum nothing abnormal was found, and here the signs of disease were slight, but well marked. Peyer's patches were much plainer than natural, and this became more apparent as the valve was approached, for here one of an inch in length and a third in breadth was prominent and brought out in strong relief, but it had not ulcerated. The impression given by the inspection was, that irritation had recently been going on in the part, but had somewhat subsided. The specimen was exhibited before the Pathological Society.

Case 19.—Bellew, a servant, aged 45, of tall stature and thin, admitted in May, 1862, with all the signs of fever in a very severe form. He had to be supported into the hospital, and though only one week ill was already densely spotted; his tongue dry and brown; eyes very much injected and expression heavy. There was also severe diarrhœa, which seemed to cease suddenly within forty-eight hours—that is, about the eighth day of the fever. From this out the attack was as genuine typhus as it is possible to describe. The spots became of the darkest, the mind very confused, with constant rambling, and passing under him. There was difficulty in putting out the tongue, and, late in the illness, hiccup. By the eighteenth day the symptoms had materially improved. The spots were gone, the tongue had expanded, and was put out better, and he took support well. It was evident, however, the fever had not resolved itself. The pulse had not fallen in proportion, nor the tongue cleaned, and he still remained heavy and at times would ramble. In this state he went on till the twenty-fifth day, when he died. There was no effort at crisis at any time nor any tympany. I was only able to examine the abdomen. The ileum had no ulceration in it, but it was very red in patches, and the more so the nearer we got to the cœcum. In this last organ the chief lesion was found, for it was ulcerated in patches, one as large as a shilling. The ascending colon had a number of small and distinct ulcers in it. The glands of the mesentery were not enlarged. It is scarcely necessary to observe that Louis' observations prove that the colon is often engaged in enteric fever, similar to what has been just described.

It appears to me these cases afford as strong a proof as the nature of the subject admits that the enteric lesion may coexist with a petechial rash, or, in other words, with typhus fever. On my own mind there now exists not a shadow of doubt of the fact, and if this be not the proper view to take of the matter, I must ask those who differ from me to explain it otherwise. What has been advanced are facts, put what interpretation on them we may. Nor would the slightest difficulty exist in giving other cases, and some striking ones have occurred within the last month; but I prefer now to glance at what others have seen; for if no one else had met similar cases to my own there would indeed be strong grounds for questioning my powers of observation, and necessarily the correctness of what I have stated. I refer, then, my hearers to the lectures of the late Dr. Todd, in which they will find some cases exactly like those given this evening—that is, the enteric type of fever attended by a copious measly rash. Some of these, too, died, and the specific lesion of the intestine was found. Again, in Chambers' "Clinical Lectures" may be found cases of exactly the same kind, and also examples of the two types of fever coming from the same room. Here, then, are two London physicians who fully bear out what has been advanced this evening, and I quote them the more readily, as they have managed to see a class of cases, which, by some strange fatality, never seem to have come under the notice of Dr. Jenner and those who agree with him; but, further, I observe that Dr. Lyons when in the Crimea met the two types of fever in the combined form, and states, specially in his work, that whilst the rash was genuine typhus the lesion often found was ulceration of Peyer's patches. In a paper, too, which has just appeared by Dr. Law on "Fever," one of the cases given is described as a typhoid case, as I believe it was, and yet the rash was a copious measly one. Lastly, the Drs. Martin, from

Berlin, whose names I have already mentioned, told me the two types of fever were commonly looked on as the same disease, and that the enteric type was there called abdominal typhus. I have not the least doubt that had more time been given I could have got further evidence in the same direction ; but enough, it appears to me, has been advanced for my present purpose. I do not, for a moment, assert that the question is settled on my side; but I do maintain that enough has been stated this evening to show gentlemen who differ from me the the need of a cautious reserve on this question, and in not allowing themselves to come to a decided conclusion till all the facts of the case are clearly before them.

Before concluding these remarks, I would advert for a moment to one other symptom which some have thought was characteristic of the enteric type of fever—I mean hemorrhage, whether from the nose or the bowels. The London physicians especially look on them in this light, but it certainly is not correct as regards Dublin. With us typhus often exhibits epistaxis, both in its earlier and more advanced stages. In the summer it is very common, particularly when the temperature ranges high; but it is much more frequent in some years than others. And, again, as regards bleeding from the intestines, I myself have put on record some thirty cases—most of them regular typhus—in which bleedings, more or less severe, occurred, and in some that proved fatal and were examined not a trace of ulceration was found. So that bleedings cannot in any way be considered as specially diagnostic of enteric fever, and I do believe the same may be said of any other symptom that might be chosen. I would repeat, however, that it is quite another matter distinguishing between the several types of fever. This can very usually be done, and ought, of course, always be attempted; but that the types of fever will often be found united I cannot doubt, and I think the time will come when the natural history of fevers—for this is really the question at issue—will be looked on in a very different light from what it at present is.

On the treatment of fever I have here little to say. As a single remedy, and in the ordinary typhus, I find balm still the best. It seems to me to act as an antiseptic, and to fulfil the indications required better than any other agent with which I am acquainted. I consider, too, that, to a certain extent, it supplies the place of wine; and this is no little matter to be able to say of it. Under its use the mortality, in spotted cases, has, I believe, been reduced to the lowest on record. But having spoken of these several points on a former occasion, as likewise the dose and mode of using it, and the precautions to be adopted, I shall not enter upon them further now.

Of the treatment of the enteric type of fever, I have only to repeat that, when seen early, it appears to me the most amenable of the several forms of the disease. I mention this because elsewhere, particularly in London, it seems to be a very fatal disease. Like typhus, it appears as if it were a more severe disease there than with us in Dublin. Though not easily accounted for, this may be so. Still my conviction is, that treatment has a more decided effect on it than any other type of fever. For myself, I use astringents, and from an early stage of the attack, and it is the dilute sulphuric acid on which I chiefly rely. This is the medicine recommended by Huss, and in the proportion of one, two, or three drachms to an eight-ounce mixture I have found it most useful. Two or three drops of laudanum are added to each ounce of the mixture, which is repeated according to the urgency of the case. It is, however, to be observed that the diarrhœa is only to be moderated, not directly checked; and this rule is the more important the earlier the disease is seen. If the diarrhœa be stopped too soon or too suddenly mischief elsewhere than in the intestines will arise. It may be in the chest, or the brain may be the organ that suffers. Several such cases have come under my notice; but though some of these were severe, none proved fatal. One, however, was so remarkable that I must give it here; for the checking the diarrhœa had, or seemed to have, the effect of altering the type of fever under my very eye.

Case 20.—Kelly, a man of 19, having a fine skin, was admitted into hospital, labouring under the enteric type of fever in a well-marked form. He presented the characteristic diarrhœa, and also the spots, and had been nine days ill. After three doses of the acid mixture the diarrhœa suddenly ceased, and was at once succeeded by symptoms referred to the head. His eyes, which before had been quite clear, became deeply injected; he complained of headache, his face flushed, he began to rave, and in the course of two days he presented the countenance of a well-marked typhus case, his tongue and lips being then covered with sordes. In this state, and when now about twelve days ill, his nose began to bleed, and this was repeated daily three times, so that he bled in all on four occasions. The first of these the bleeding was much the most, and they were all so obviously beneficial that they were not interfered with. The patient made a good though very slow recovery. There was no recurrence of symptoms referable to the intestines. I have seen several instances like the one just given, but none so striking, and none which proved fatal. When, however, any similar instance occurs, it may be assumed that the case is quite within our control.—*Medical Press and Circular, June* 20, 1866, *p.* 647.

3.—ON THE USE OF STIMULANTS IN THE TREATMENT OF TYPHUS FEVER.

By Dr. T. J. Maclagan, late Superintendent Dundee Royal Infirmary.

The question of stimulation is one which should be inquired into less in the spirit of an economist than in that of a physician. The rule which should guide us in the administration of stimulants should be to increase as much as possible, by every available means, the patient's chance of recovery. If, by the free administration of wine or spirits, we think that this (however hopeless the case may seem) is increased by one iota, the benefit of that chance we are bound to give him. The real question at issue is the quantity which it is proper to administer. To give on paper an exact or even fair account of the extent to which stimulants have been used in a given number of cases is exceedingly difficult. To say simply that a case was stimulated, or that so many out of every hundred were stimulated, is to convey the idea that the writer has been in the habit of using stimulants in the treatment of his cases, but conveys no information regarding the extent to which they may have been employed. Stimulation may mean two ounces of wine or twenty ounces of brandy. Neither does it serve any useful purpose to say that so many patients were treated, and that such and such a quantity of wine was consumed, being an average for each of so many ounces, when in the calculation are included those who were not, as well as those who were, stimulated. The ages of the cases treated must also be taken into account, old people requiring a much larger quantity of wine and spirits than young. It seemed to me, also, that to take the total quantity of stimulants consumed by each patient was a method likely to lead to inaccuracy, as many cases might have the period of stimulation much prolonged in consequence of the existence of some other weakening cause. To take the average daily allowance seemed also open to objection, as being likely to lead to an under-statement of the extent to which stimulation was carried, besides being more difficult of application than the plan which has been adopted of taking the largest quantity taken continuously for twenty-four hours by each of the cases, adding these together, and dividing the sum total by the number of cases stimulated, and so getting the average of the maximum daily allowance. Of course, in making such a calculation, the cases not stimulated are entirely left out. This method possesses the advantage of showing better than any other, I think, the extent to which stimulation has been carried; at all events, the average is not understated. The result will be more readily seen by dividing the ages into fewer periods than by adopting the division into quinquenniads. I shall, therefore, divide them into three classes, those under 25, those from 25 to 50, and those from 50 upwards.*

It seems that of 2476 cases, 1107, or 44·7 per cent., were stimulated; the average of the maximum quantity given to each case *per diem* being 3·19 ounces of wine, and 2·6 ounces of spirits. Of those under 25 years of age, 25·8 per cent. were stimulated, the average for each being 3·5 ounces of wine, and 1·09 ounces of spirit; of those from 25 to 50, 73·7 per cent. got an average of 3·2 ounces of wine, and 3·07 ounces of spirits; whilst of those above 50, 95·3 per cent. were stimulated, the average maximum quantity being 2·1 ounces of wine, and 5·7 ounces of spirits.

It will be observed that during the last three years, but more markedly during the last two, the percentage of cases stimulated under 25 years of age is considerably less than during any of the years which precede; the quantity of stimulant given is, at the same time, just about the average; the death-rate being slightly below it. It will also be seen that in the other two periods of those years, there is a considerable increase in the quantity of spirits administered, whilst the amount of wine given is below the general average for the same periods; both the percentage of cases stimulated and the death-rate being very near it.

These two facts indicate the principles on which treatment has been conducted. In young people, as a rule, stimulation is not called for to any great extent; whilst in old people, the contrary holds good. In the young, the danger is not so much from failure of the cardiac action as from the head-symptoms; death in them is due less to asthenia than to coma. It is comparatively rare to find in a young patient the almost imperceptible cardiac impulse and inaudible systole which form so frequent and anxious a feature in the cases of old people, into the mode of production of whose death asthenia enters largely. True it is that every now and then a case occurs in a young person demanding a pretty liberal administration of stimulants, just as one occasionally meets cases advanced in life characterized by unusual mildness; these are exceptional, but serve well to show the fallacy of laying down any general rule of treatment founded on the ages of the patient, and making no allowance for individual peculiarities. When free stimulation is required, spirits are more convenient than wine; an ounce of spirits represents a greater amount of real stimulant than double the quantity of wine. It was not a very uncommon thing, in treating the above cases, to order 12 ounces of spirits *per diem*, half-an-ounce every hour; seldom has stimulation been carried beyond this for more than a few hours at a time. The mode of treatment to which the cases included in these statistics, at least those admitted during the last three years, were subjected, may be characterized as mainly dietetic in the early periods of life, and freely stimulant in the more advanced periods. The success which attended it may be, and very likely is due in part to some of the agencies noted in an earlier part of this paper; but the fact remains that, with this treatment, there is a death-rate considerably below that of most other hospitals. A good deal has of late been said about the harm likely to accrue from over-stimulation in typhus. My own very strong impression is, that whilst a deal of harm may result to young people from the indiscriminate adoption of this method of treatment in all cases, a great deal more injury would result to elderly patients from its abandonment, and the substitution of a non-stimulant plan. The only truly rational plan is to do with typhus as with every other disease,—treat each case on its own merits, but have constantly before us the lessons taught by experience and observation, that age increases immensely the fatality of the disease, and that in elderly persons death is threatened by failure of the heart's action to a much greater extent than in young people, in whom the head-symptoms must be looked to as the chief source of danger.—*Edinburgh Medical Journal, August* 1867, *p.* 155.

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* Dr. Maclagan here introduces a Table which shows the amount of stimulant given in his cases.

4.—ON THE USE OF OPIATES IN CONTINUED FEVER.

By Dr. N. G. Mercer, formerly House-Surgeon to the Clayton Hospital and Wakefield Dispensary.

[There occasionally occur cases of fever in which the administration of an opiate becomes absolutely necessary, in which the narcotic has a remarkable therapeutic power, and gives to the sinking energies of life their last chance of rallying. Hence it becomes necessary to define the cases in which its use is admissible. With this view the following case is related. The patient was a pensioner, 47 years of age, one of many in a large Irish lodging house, which was crowded and unhealthy to the last degree.]

His case was from the first attended with the gravest typhoid phenomena. I had attended him for nearly two weeks, and the symptoms were growing daily more alarming, although an exception must be made in favour of an attack of bronchitis, which had been severe from the beginning, but had undergone partial improvement. The delirium had not abated for three days, and its active character frequently necessitated restraint to keep the patient in bed. The pulse-rate had gradually risen in frequency, and had reached 144 per minute. Great wakefulness had existed from the first, but in the middle of the second week had become so extreme, that out of twenty-four hours scarcely a whole one of sleep was obtained as the aggregate of many short-lived snatches. The bowels had been alternately loose and constipated, the evacuations invariably presenting the peasoup characters. The following were the signs and symptoms present on the thirteenth day of the fever, on which, as determined by treatment, the turning point seemed to take place :—The patient was almost completely deaf, and, when asked in a loud tone to show his tongue, seemed to comprehend nothing. A view of it was, however, obtained by mechanically separating the jaws, and it was seen to be black-brown in colour and perfectly parched, while the teeth were covered with sordes. The respiration was hurried from the bronchitis, although this did not exist to the same extent as a few days before. The abdomen was literally blown out with tympanitis ; the patient was continually fumbling with the bedclothes, and muttering inarticulate sentences. The evacuations were and had been involuntary for three days. During two entire days and nights, at the very least, his wife declared that the wakefulness had been without a moment's intermission. All the symptoms present in general, although this last and the existing character of the pulse in particular, indicated and warranted the experiment of an opiate. That character of pulse is hardly to be described ; once felt it is perceived to be remarkable, and is not forgotten. In this case it was not possible to estimate the number of beats per minute, on account of its extreme rapidity, but chiefly—thus completely conforming to that description of pulse where opium is highly serviceable—because of its weak, fluttering, confused, and indistinct character. The beats appeared to run into one another, without that appreciable interval present in all other variations of the pulse, whether in health or disease. Such are the circumstances under which we are culpable in withholding an opiate, as the last chance of saving our patient's life. A very mild one in this case was found sufficient to compass the object in view. A pill containing half a grain of opium was to be given at bedtime, and if this seemed to have the effect of slightly lessening the delirium and restlessness, a pill of like strength was to be administered in two hours. If a tendency to sleep happily supervened, the second pill was to be omitted.

From the very hopeless nature of the symptoms present on the afternoon when this treatment was adopted, the prognosis I gave was to the effect that the patient would certainly die ;

and at my visit on the following day I did not dare to expect an improvement in the symptoms, although in cases less severe I was well acquainted with the beneficial influence of opium. My disappointment, however, was as great as it was agreeable. The patient's wife informed me that after giving him one pill only he showed a strong disposition to sleep in less than half an hour, and that accordingly the other pill had not been administered. Soon after he fell into a profound slumber, and at my visit he was calmly asleep. At intervals he was disturbed, in accordance with my instructions, for the purpose of giving nourishment. Under the influence of the half grain of opium, the pulse, too, had undergone a most notable change, having fallen to 132 per minute, and its beats having grown perfectly regular. Although I still gave a cautious prognosis, the symptoms had become, in fact, a little hopeful, while the day before they were desperate.

The subsequent history, which might be termed one of uninterrupted improvement, may be summed up in a few words. Ever since the eventful night when the soporific was given, the patient enjoyed nightly a few hours of refreshing sleep. Delirium thereafter formed a very slight feature in the case, and his deafness went also gradually away. What tended chiefly to retard recovery was an immense sloughing bedsore, which burrowed amid the muscles of the nates, and exposed the crest of the ilium. Under suitable applications, however, this was towards the close of the fourth week converted into a healing, granulating ulcer. Ammonia and bark were early and freely given after the active symptoms of fever had abated, and our Dispensary contributed towards enriching his by no means over-liberal diet, for he took daily after the third week three ounces of cod-liver oil with a keen relish, and to his great benefit. Soon after I lost sight of him, having been told at my visit that he had gone some miles into the country, where, no doubt, he finds the change of air, lodging, and surroundings of much advantage to his weakly condition.

In conclusion, I have only to observe that this case, like many others of the same fever, refused to conform to that rule of limitation as to time which systematic authors speak of in this disease, while there could be no doubt whatever of its nature. Small opiates only were hazarded on account of the degree of bronchitis present ; but even these served to illustrate the salutary and beneficient agency of the narcotic, controlling irritability, adding force to the action of the failing heart, and well establishing the medicine's claim to be called, as it has been by Dr. Christison, the "best drug of all."—*Medical Times and Gazette, Sep.* 28, 1867, *p.* 346.

5.—CEREBRO-SPINAL FEVER: ITS TREATMENT BY MEANS OF ICE.

By Dr. JOHN CHAPMAN, Physician to the Farringdon Dispensary.

Before we can act scientifically for the removal of the proximate cause of the group of symptoms, consisting of trismus, retraction of the head, opisthotonos, pleurothotonos, rigidity of the recti muscles, emprosthotonos, contraction of the muscles of the extremities, convulsions, and subsultus tendinum, we must first assure ourselves, of course, what the precise nature of that cause is. In classifying the symptoms of cerebro-spinal fever, I have assumed it to be hyperæmia, proceeding possibly to the extent of inflammation, of the spinal cord, but I am aware of the existence of a doctrine concerning the proximate cause of muscular contraction which is wholly inconsistent with that generally held on this subject, as well as with the practical application of it which I am now about to advocate. I refer, of course, to the peculiar views concerning the proximate cause of muscular contraction, and of pain, which have been elaborately and ingeniously expounded by Dr. Bland Radcliffe. In a paper entitled—"The Treatment of Epilepsy: Principles and Practice," read to the Medical Society of London, and afterward spublished in this Journal (March 27th, April 4th), I briefly explained Dr. Radcliffe's views, and adduced what appears to me conclusive reasons for believing them to be confuted by a large number of physiological, pathological, and therapeutical facts. On this occasion, therefore, while referring students interested in the subject to that paper, I shall assume, without further discussion, that all excessive muscular contraction is due to excessive energy in those nervous centres which preside over the muscular system. Now, this being so, it is obvious that the only rational way of treating lock-jaw, retraction of the head, and other tetanoid states, as well as convulsions and subsultus tendinum, is by lessening the energy of the nervous centres producing those phenomena. Can we do this? I affirm that we can, and, notwithstanding the difficulty of many thoughtful men in believing it possible, by applying ice along the spine, to influence the circulation of the spinal cord at all, and the fact that, up to the present time, this method has, with a few exceptions, been wholly disregarded or unknown, I believe that the time is not far distant when it will be counted on by medical practitioners as one of the most powerful remedial agents at their command for subduing hyperæmia or inflammation of the spinal cord.

Some physicians who are disposed to recognise the reality of the results which I have obtained by the application of heat and cold along the spine, and who cannot believe those results are due to the direct action of these agents on the spinal and sympathetic nervous centres, are inclined vaguely to regard them as phenomena of reflex action. (One of the most eminent physicians recently asked me if I had tried whether applying ice over the stomach would not produce the same effect as that produced by it when applied along the spine!) I am satisfied, however, that this view is untenable and quite inconsistent with many ascertained facts. But I cannot here enter on a discussion of this subject, and must content myself with observing that whatever mode of accounting for the results in question may be most scientific the existence of them is indubitable, and, as in a practical point of view, the question of their reality is the all-important one, I will, before proceeding further, adduce evidence that various morbid conditions of the muscular system analogous to those commonly associated with cerebro-spinal fever, may be most effectually abolished by the application of ice along the spine.

I have already cited the evidence of Dr. Todd, and I may observe in passing that he was one of the few physicians who never doubted that the application of ice along the spine acts directly on the spinal cord. In the Medical Times and Gazette, March 12th, 1864, Dr. Edmunds reports a case of Emprosthotonos (?) cured by the use of ice to the spine. The facts of the case are briefly summarised as follows :—An intelligent married woman, aged 28, mother of two children, and who was "altogether free from flightiness or hysteria," went to bed, January 21st, quite well, but on rising next morning at 7·30, found her fingers were slightly benumbed ; they shortly afterwards "turned in." The "turning in" quickly extended to the hands, rapidly increased, and caused a cramp-like pain in the arms. Her legs became affected in a similar way but less severely. The morbid muscular contractions steadily increased during the day. At ten p.m., "her fore-arms were hard and rigid, and the wrists were so fully flexed that her hands were forced open," and "the backs of her wrists were fixed close to her mouth. . . . Her neck became turgid and the respiration somewhat constrained. . . . Her spine was bent forward, and her knees were bent and drawn upwards. . . . She could not stand, and her legs were about as much cramped as in a bad case of cholera. . . . There was an expression of great anxiety and pain upon the face, and the lips were sufficiently retracted to show the teeth. . . . She could swallow by making a great effort to ignore the tearing cramp-like pain which occupied her, and which forced from her a continual moaning scream." Between eleven and twelve a.m., Dr. Griffith ordered her some aperient medicine ; at one p.m., Dr. Webber, who saw her in consultation, prescribed "sulphuric æther and antispasmodics at once, and a dose of morphia to be taken a few hours later if the pain should not abate." She grew worse; the morphia was given, and, afterwards, "all her symptoms were aggravated." At ten p.m., Dr. Edmunds was called in. He ordered the administration of a purge consisting of croton-oil and calomel, an enema of turpentine and castor-oil, and the application of ice along the whole length of the spine. Cold was thus applied continuously during "seven hours," when all her severe symptoms had disappeared. The screaming and moans were at once mitigated by the ice, and in about half-an-hour ceased entirely. In two hours her feet became flexible. About eight a.m. on the 23rd the spasms had so far subsided that she could open her hands, but the numbness remained. On the morning of the 24th the numbness entirely disappeared, and she had no return of cramps or spasms. . . . The patient herself ascribes the relief of her dreadful pain and cramps entirely to the use of the ice," says Dr. Edmunds, who adds,—"and although it is difficult to distinguish between the mere *post hoc* and the *propter hoc*, yet I cannot but believe, if ever I saw a life saved by a therapeutical application, that this patient's life was saved by the use of the ice in this way to the spine."

The Australian Medical Journal for March, 1866, contains a report off a case of tetanus treated successfully by the application of ice along the spine. In this case, chloroform and atropine, three doses of each, were given; but no one reading the report will fail to be convinced that it was the ice along the spine which saved the patient. "In two hours after its application," says the report, "the spasms had considerably and decidedly diminished. . . . Our entering the room caused such an excitement in the patient as to bring on all the symptoms that had been in abeyance for four hours and a-half. The application of the ice was renewed, and in twenty minutes the spasms had disappeared."

On the 24th of February, 1866, I received from Dr. Roberts, of Northallerton, Yorkshire, the following letter:—"As an old fellow-student, I have watched with more than ordinary interest your treatment of diseases by variations in temperature. Have you ever tried it in tetanus? I have a case of traumatic tetanus which has remained almost stationary for a fortnight, and I have a notion that one of your ice-bags applied to the spine would be of service. If you have any experience, I should be glad to avail myself of it, or if you have any theory on the subject, I should be glad to test it. I have given the patient belladonna and morphia, but to no purpose. The tetanus has supervened on a large scalp wound over the occiput, exposing a large-ISH surface of the bone." I sent immediately to Dr. Roberts a spinal ice-bag of suitable length, also a "lumbar ice-bag," and recommended him to apply ice along the whole spine, and at the same time, by means of the lumbar-bag across the occiput—from ear to ear. I begged him not to let the presence of the wound deter him from placing the lumbar-bag as advised, and to keep both bags well supplied with ice, and applied continuously until the disease should be subdued. In reply to my inquiry as to the results of the treatment, Dr. Roberts wrote me (May 20, 1866)—"I am glad to say my case of tetanus got quite well. I have not had time to put my notes of the case into shape and send them to the Lancet, but I hope to be able to do so very shortly." I regret to say this "hope" still remains unfulfilled.

In the Medical Times and Gazette of June 16, 1866, Dr. John W. Ogle reported a case of "tetanoid convulsions overcome for a time by application of ice to the spine." A boy two years old was brought to St. George's Hospital, at eleven a.m., February 16, in a state of general convulsions. Dr. Ogle "ordered an enema with castor-oil and turpentine to be given at once, and the gums to be examined for the purpose of seeing if they required lancing. It was found impossible, however, to open the mouth. He then ordered an ice-bag to be applied, and kept in close contact with the back, the entire length of the spine. . . . Gradually the convulsions abated, and the child was considered by the apothecary well enough to be allowed to go home in the evening." On the following day, the child being "evidently not so well," was again brought to the hospital, but was placed under the care of Dr. Pitman, who prescribed grey powder, antimony and salines, and did *not* use ice: convulsions recurred, and proved fatal, February 19. Dr. Ogle, commenting on this case, observed:—"The substantial relief from the ice application was not a little interesting, and speculation upon the probability of a greater and more permanent benefit from a longer application of the ice could not be resisted."

Under the head of "Gleanings from Allopathic Literature," The British Journal of Homœopathy, for April, 1866, says:—"Falconer related the case of a boy, aged 11, attacked with severe tetanus, who recovered under the constant application of ice to the spine for thirty-two days. Howard records a case of opisthotonos in a man, aged 25, cured by ice to the spine." I have not ascertained what "Allopathic" journals these cases were gleaned from.

In the Medical Press and Circular of March 27th last, Mr. Hargrave reports a case of tetanus, in which great relief was given by ice applied to the spine, and in which a cure would probably have been effected, had a properly made spine-bag been used. The ice was applied in an ox gullet, and during its application the patient was singularly benefited. Unfortunately, the gullet burst in the night, and ice was not afterwards applied, Mr. Hargrave being evidently ignorant that india-rubber spinal ice-bags, made under my direction, are readily procurable in Dublin. During the application of the ice, "the muscles of the lower jaw were so relaxed, that the patient could open her mouth and take some nourishment and medicine, from which," says Mr. Hargrave, "I augured favourably."

In the number of this journal for May the 8th of this year, I published a case of what I called "Tetanoid Paralysis of the Four Extremities," accompanied with swelling, ecchymoses on the feet, and extremely heightened sensibility, which was completely cured by the continuous use of spinal ice-bags.

On the 12th ult., at the Farringdon Dispensary, I prescribed for a patient, Margaret Macgrath, aged 14, who was suffering from scarlet fever, with inflammation of the throat. The fever subsided, but on the 19th the patient came to me with the head violently twisted round, so that the face looked over the left shoulder; the cervical muscles were tense, extremely tender and sore, the tenderness extending to the shoulders, and were seemingly so swollen, that I thought, at first sight, the appearance was due to glandular enlargement. Being satisfied that the case was simply one of persistent spasm—a sequela of the influence of the fever on the spinal-cord, I ordered ice to be applied along the cervical and upper part of the dorsal spine. Within thirty minutes of the beginning of the application, which was continued two hours, the patient could move her head in any direction, and the next day all tenderness was gone.

The above statement of facts justifies, I think, my conviction that in the present state of medical knowledge our greatest chance of curing tetanus and tetanoid affections consists in the proper application of ice. I am aware that a number of cases were published in the Lancet a short time ago, which seemed to prove that ice to the spine is of no avail against this deadly malady. But the writer gives no particulars of his method of applying the ice, and, in the absence of these I can attach but little importance to his conclusions. It will be observed that in the case cited above, under the care of Dr. Roberts, I directed that while ice was applied along the spine, it should also be applied in a separate bag across the occiput, notwithstanding even that it had to be placed directly across a wound in which the bone was exposed. I consider the adoption of this plan in all cases of tetanus of the very utmost importance, and think it probable that in those fatal cases in which ice may have been applied continuously along the spine, it was not applied at all across the occiput. When ice is applied along the spine, and especially over what is called the cilio-spinal region, the vasomotor nerve centres controlling the cerebral arteries are subject to its sedative influence, and, hence, those arteries become dilated, and are thus enabled to supply the encephalon with more blood than before. In this manner, as I conceive, the medulla oblongata is maintained in a hyperæmic state, at the same time that the spinal cord is subject to the sedative influence of cold. Such being the case, it is obvious that that influence may be partly neutralized by the preternatural energy of the medulla-oblongata propagated downwards; while, of course, the nerves arising from it, including the motor divisions of the fifth and seventh, and the muscles they supply, if not kept as active as they were before ice was applied at all, are, at any rate, scarcely likely to be rendered quiescent. Much, however, will depend upon the way in which the ice is applied. If, for example, it be restricted to the lower cervical and upper dorsal vertebræ (the cilio-spinal region), there will probably be a maximum afflux of blood to the head; but if the ice be continued upwards over each cervical vertebra to the base of the skull, the sedative influence exerted on the spinal cord is likely to extend in some degree to the medulla-oblongata. The considerations here advanced afford an explanation, which is perhaps the true one, why in certain cases of tetanus ice applied along the spine has been found useless, and, at the same time, indicate that, as a general rule, when ice is applied along the spine in such cases it should be carefully kept in contact with the uppermost cervical vertebræ, and that it is always indispensably necessary to apply it in a separate bag across the occiput *simultaneously.*

The practical application of the foregoing facts and observations to the treatment of cerebro-spinal fever scarcely needs indicating: trismus and retraction of the cervical muscles when occurring in the course of that disease, should be treated in the manner just described, care being taken by incessant watchfulness that air (a bad conductor of heat) is not allowed to accumulate in the upper end of a spine-bag, or in the side next the head of the lumbar-bag placed across the occiput, and that both bags be kept well supplied with ice. If they are allowed to remain on the patient when they cease to be cold they will do positive harm; for, as india-rubber, like air, is a bad conductor of heat, when it is applied to the skin, it prevents, of course, that passage of heat from it which would otherwise take place. This method of treating the tetanoid symptoms frequently associated with cerebro-spinal fever is fortunately calculated to subdue, at the same time, that morbid state of the cerebro-spinal axis which constitutes, so far as our positive knowledge extends, the very essence of the disease.

In those cases in which convulsions, partial or general, supervene the treatment should be the same; for though convulsive paroxysms apart from cerebro-spinal fever may be effectually treated by ice along the spine only, the action of the ice is always more complete and certain if, *when used during the fits*, it be applied across the occiput also ; and even though tetanus or tonic spasms of the cervical muscles may not occur in certain cases of the disease in question, there will probably exist strong reasons against so using ice along the spine as to facilitate the flow of blood to the brain in fuller streams than before without taking care to keep it under the sedative influence of cold at the same time. I need not enlarge here upon the efficacy of the spinal ice-bag in the treatment of convulsions, as I adduced ample evidence of its power in this respect in the paper on Epilepsy, already mentioned. I may, however, appeal to the testimony of Dr. Routh and Dr. Rogers, that in cases of both infantile and puerperal convulsions, they have found that the spinal ice-bag arrests them, and may add that I am in possession of a large number of facts proving, not only that these maladies, as well as epilepsy, are amenable to this method of using cold, but that laryngismus stridulus, ordinary night cramps, and the cramps of cholera, and those occurring in some cases of sea-sickness, may be completely subdued in the same way. I conclude, therefore, that all spasmodic, convulsive, or epileptoid affections incidental to cerebro-spinal fever may be most surely controlled by precisely the same treatment as is necessary, not only for the tetanoid group of symptoms, but for those to be now immediately referred to, and which I regard as no less significant of morbidly increased activity of the spinal cord.

In my pamphlet on Sea-sickness, and in a paper on the Sickness of Pregnancy—read to the Medical Society of London, I have developed my views concerning the pathology of nausea and vomiting, and in my work on Diarrhœa and Cholera, the pathology of those diseases, and the mode of production of an abundance of pale urine of low specific gravity, are also fully discussed, and, as experience emboldens me to believe, correctly explained. I shall, therefore, content myself here with the statement of my conviction that I have proved that nausea, vomiting, diarrhœa, and preternatural abundance of pale urine *can only occur* by virtue of a preternaturally vigorous action of the spino-ganglionic nervous centres presiding over the organs implicated, and that that excessive activity can be restrained most scientifically and most satisfactorily without the evil influence of narcotics, stimulants, or other drugs, by the use of the spinal ice-bag.

In the group of symptoms of inflammation of the spinal cord and its membranes, I have classed, besides those just referred to, *erotic desires*, *priapism*, *subsultus tendinum*, *swelling of the tongue*, *petechiæ*, and *vibices*. It will probably be admitted at once, by each of my readers, that the first three of these disorders are expressions of disorder of the spinal cord, and I shall, therefore, only observe respecting them, that I am assured by experience that they may be treated successfully by the spinal ice-bag, and that, if in cases when subsultus tendinum is present, the physician will apply the bag, and watch the results, he will generally be surprised and gratified, not only by the rapidity with which the affected muscles will be soothed into quiescence, but by the generally refreshing and vivifying influence which the ice thus applied exerts.—*Medical Press and Circular, July* 31, 1867, *p.* 97.

6.—SULPHUROUS ACID FUMIGATION.

By Dr. James Dewar.

The following cases, as illustrating the advantages of this mode of treatment, will no doubt prove so interesting as to induce others to submit the system to the test of intelligent experiment, and so judge for themselves.

The Rev. —— for fifteen years had suffered from disease of his throat, rendering preaching laborious. General health "below par;" obliged to change his "underclothing" between services, so uncomfortable was he from perspiration; had tried everything, without good result; began the sulphurous acid inhalation, (besides "fumigating" twice or thrice a day); improvement immediately commenced, and a cure was speedily effected. General health has since greatly mended, and finds his duties now no burden.

Rev. —— called for me to inquire about sulphurous treatment of throats. He had suffered from his ailment for many years, which made him quite pluckless, and afraid to undertake duty, from a feeling of nervousness. I examined his throat, and, without any remark, applied the spray. He instantaneously felt relief, and said that he could command his voice in a way he had not done for long. He asked me what I had used, and was surprised to learn that it was sulphurous acid, as he had believed that the remedy of which he was in search was sulphur *pur et simple*. The last accounts I had from him were satisfactory.

Rev. —— had been a victim to this malady for twenty years; found himself very ill able to discharge his duties, from want of vigour and unconquerable nervousness, which especially laid hold of him when he had occasion to pronounce words containing certain consonants. Was in a state of agitation at the idea of having the spray applied, but, nevertheless, he at once clapped his hands, and exclaimed "Delightful!" At the end of a month he returned to London, and is now in perfect health, and fit for any duty. He conjoined fumigation with the use of the spray. It is interesting to note that each of these gentlemen used the same figure to illustrate the feeling of relief which they had obtained, which was, "that something which for years had been loose in the windpipe had suddenly been 'braced up,'" which, doubtless, was no inaccurate explanation of the real change which was effected upon the vocal chords.

Miss —— came home from London last season, changed somewhat in her appearance, and showing symptoms which naturally made her parents anxious. She had lost flesh, had a tickling cough, and a tendency to flush. Upon examination, the chest proved all right, but the condition of the mucous membrane of the pharynx and larynx was evidently sufficient to account for the cough at least. The spray was applied a few times, when the disease was quickly eradicated, and a return to her wonted health has been the satisfactory issue.

Mrs. ——. This lady had for several years been obliged to winter in a southern climate, on account of liability to catch cold, and general delicacy of health. I saw her early last summer, and, from the satisfactory state of her chest, was led to believe that she was likely to be benefited by "sulphurous acid medication." Both processes were adopted with immediate advantage. She rapidly regained her health, and when I last heard of her, she was taking on flesh, and felt strong and well, She had persistently put in practice the plan recommended, and was to continue it during the present winter.

Miss ——. Last autumn I was asked to see a young lady from Edinburgh, whose general condition had recently given her friends much anxiety, and when I visited her it seemed to me that her appearance, coupled with what I was told were her symptoms, fully justified the apprenhensions entertained concerning her. The family history being somewhat ambiguous, I was fully prepared to find something wrong in the chest, but in this I was very agreeably disappointed. The appearance of the fauces, however, was such as to account for everything, as I thought, and accordingly the spray was applied with the usual good effects, and under the influence of this and fumigation the patient has progressed marvellously. I have not seen her again, but the last report said that she was so well as to think confinement to the house at night unnecessary and very irksome.

A very interesting case of cure of an obstinate laryngeal affection of many years' standing has lately passed under my observation. Mrs. —— had long suffered from this disease, which had baffled all attempts at remedy, although she had consulted the most eminent medical men in different parts of Europe. Some weeks ago I had occasion to see her, and took the opportunity of recommending and applying the spray. I left the apparatus with her, enjoining its use four times a day, and she writes me to say that her throat is quite well, and that she enjoys a sense of returning vigour which she had not hoped to attain.

Last week a young lady, who used to sing very nicely, called for me to ask whether I thought the remedy in question would benefit her, as for months she had been unable to sing as was her wont, and besides had suffered from uneasiness in the chest, which naturally gave her some concern. Her case appearing to be a suitable one, the spray was applied at short intervals during a *séance* of five minutes, and she arranged to come again each morning to have the process repeated a few times. When I next saw her, she joyously said that she was quite relieved of every feeling of discomfort, and "that she had sung as well last night as ever she had done!"

Chronic Bronchitis and Emphysema.—In this class of diseases sulphurous acid medication is attended by speedy and signal benefit. In whatever stage I find my patient, I at once proceed to apply the spray, which, by loosening the expectoration, facilitates its expulsion, so that the chest can be wonderfully cleared of it in a few minutes. Cough is no doubt excited, but this does not deter patients from its use, finding as they do, that they are more than repaid for the temporary inconvenience.

— F., now a post-runner, formerly a miner, has been asthmatic for many years. In January last his condition became much aggravated, and, after ten weeks' struggle, he was obliged to abandon his duty. I found him terribly oppressed, his attempts at expectoration being distressing because unsuccessful. The spray enabled him at the outset to begin the process of clearance; his improvement under the conjoined remedies was progressive, and in the course of five days there was scarcely a râle to be heard in his chest. The spray was applied twice a day, and his room was filled with "fumes" every four or six hours. He has long since gone back to his work, and he told me yesterday (April 10, 1867) that he is in better "wind" than he has been for ten years.

J. P. had been ill for weeks before I saw him, his breathing difficult, and his efforts at expectoration almost fruitless. I persevered, pumping in the spray at intervals for some twenty minutes, until his chest was much relieved and almost noiseless. Next day this was repeated, which completed the cure.

I could easily multiply the number of cases such as those just detailed, but this is quite unnecessary, as like favourable advantages have many times accrued in individuals *otherwise* abandoned to hopeless suffering. That this agent exercises a *really curative* influence upon the pulmonary mucous membrane, the rapid improvement in the health, and the comparative immunity from other attacks of bronchitis in those so operated upon, sufficiently testify, while the simplicity and perfect safety with which the process can be employed render it peculiarly suitable for the desired purpose.—*Medical Times and Gazette, May* 25, 1867, *p.* 548.

7.—REMARKS ON THE ACETIC ACID CURE FOR CANCER.

By Dr. M'Gregor, Medical Superintendent, Barnhill Hospital.

[The first indication of the presence of cancer in the case which forms the subject of the following article, was an obscure hardened swelling deep in the substance of the left mamma. There was an occasional lancinating pain. When first seen by Dr. M'Gregor there was no retraction of the nipple, no discoloration, no history, hereditary or otherwise, to lead to the diagnosis of malignant disease. In a few weeks after the case was first seen by Dr. M'Gregor the swelling had increased considerably in size, had become harder, and the lancinating pain more frequent and severe. There being then no doubt as to the malignant nature of the case, it was determined to give Dr. Broadbent's method of cure a fair trial.]

I began the injections by means of a Wood's syringe, using a dram of the acid, of the strength one part of strong acid to three of water, or one in four. This was repeated several times, every second day, the dram being generally divided equally into two portions and injected in two distinct parts of the tumour. After a week's trial in this manner, I found that the disease, if not aggravated by the remedy, was certainly not improved by it, for now it seemed to be rapidly spreading, the tumour becoming larger, and the pain increasing in severity. Under these circumstances, I increased the quantity of injection to two drams, and also the strength of it to one of acid to two of water, or one in three, instead of one in four as before. So hard and dense was the tumour (scirrhus) that I found great difficulty in injecting so much as two drams into it by Wood's syringe. I therefore employed a small trocar and canula, generally giving the patient, who was a little nervous, chloroform during the operation, the acid being injected as before into different parts of the tumour on each occasion. In this way I persevered for a fortnight more, but only to meet with disappointment. The disease spread rapidly, soon involving the whole breast, and ulcerating through the skin at one or two points of it, so as to leave me much too little skin to cover the wound, when, after the failure of the injection, I felt compelled to remove the tumour. This I did (with the able co-operation of my assistant, Dr. Bain,) along with a couple of swollen and suspicious glands in the axilla. She made a good recovery, the wound healing up satisfactorily, notwithstanding the lack of skin (owing to previous ulceration) to cover it, with the exception of a small point which, instead of closing up like the rest, remained in an angry and irritable condition. I tried every means at my command to make this heal. On the contrary, it soon gave signs of spreading and becoming larger. It was soon apparent that the cicatrix was opening up rather than healing firmly, as it should have done, and that the dread disease was not going to yield to all the measures employed for its extirpation.

To this small ulcerated surface clearly relapsing into a diseased state, I now began to apply dressing of lint soaked in the *acetic acid*, of the strength of one in three as before, thinking that if of any use at all in battling with this disease, a better opportunity of noticing its effects could not present itself. For here was an ulcerated surface of the size of a penny piece, evidently relapsing into a diseased state, notwithstanding the removal of the entire breast; and if the acid has the power of effecting that metamorphosis of tissue attributed to it by injection into a cancerous tumour, it ought to influence a raw surface like this affected by the same disease, when directly applied to it, in a similar manner. Yet, notwithstanding its constant application in the manner indicated, the disease continued to spread till the whole cicatrix was involved in it. Unfortunately the same disease now began to show symptoms of having attacked the liver and intestines, and if I had any hope before of saving the patient it now disappeared. At this date, I am sorry to say, she is rapidly sinking.

On examining the excised breast, by making incisions into various parts of it, it was noticed that at the points where the acid had been injected, the cellular tissue had a dark, broken-down, sloughy appearance for about a quarter of an inch on either side of the points of introduction of the acid. Under the microscope, the portion of the breast unaffected by the acid presented the usual indications of scirrhus. Where affected by the acid, there was nothing made out but a mass of disintegrated, broken-down, cellular tissues that presented nothing remarkable.

During the application of the acid on the lint dressing to the ulcerating surface, there was noticed on each removal of it, a thin layer of a dirty gray, cheesey-looking matter underneath the dressing. This could be scraped off by the nail in small soft scales or rolls, leaving bare patches of the ulcer exposed, and was certainly due to the action of the acid, for where it was not applied, this cheesey layer did not exist. It did not seem to be due either to its action on the discharge, which an ulcerated surface will always more or less produce, but rather to the action of the acid on the tissue it came in contact with. This made me hope at first that the diseased tissues might be broken up and got rid of in this way, so I continued assiduously to apply the acid to the affected parts, clearing away this cheesey matter and dressing the sore afresh three times a-day, so that the acid might, on each application, have a new surface to act upon. In spite of all this my efforts proved unavailing and the disease went on its rapid course, seemingly unchecked by this treatment.

The apparent extent of the effect of the injections, as noticed in the breast after its removal, seemed to me so trifling in comparison to the mass it had to contend with, that I could not help doubting its efficacy when the disease had assumed anything like considerable proportions; even the size of a walnut, unless, indeed, it progressed more slowly than malignant disease usually does. The injection of the acid will always have a tendency, as in this case, to produce irritation and pain, if used of such strength as to affect the tissues, and this must *hasten* the progress of the disease when it fails to check it. I am confident that had I removed the breast in this case a few weeks sooner, instead of putting off time trusting to the injection, there would have been a much greater likelihood of the breast healing thoroughly after the operation, and keeping well, though the disease having attacked in ernal organs, the result to the patient would ultimately have been the same.—*Glasgow Med. Journal, June* 1867, *p.* 52.

8.—ON THE USE OF ALCOHOL IN DISEASE.

By Dr. Samuel Wilks, Physician to and Lecturer on Medicine at Guy's Hospital.

I am not going to enter upon the much vexed question as to the necessity for the daily use of wine or spirit; but I will say that the usual test for the advantages of its use is one which I conceive to be in most cases utterly valueless. Alcohol, remember, although an excitant, is a sedative to the nervous system—is, in fact, an anæsthetic. A drunken man may be injured in such a way as to have all his teeth knocked out in a brawl, yet apparently not perceive the injury, and be utterly unconscious of the occurrence when he has returned to soberness. The argument, therefore, that a man feels better after his glass or two of grog would be equally applicable to the case of the Turk who feels better for his opium. His feeling better simply means that he has got rid of his unpleasant sensations, whether these be moral or physical; he "drowns his troubles in the bowl." If a man engaged in the practice of his profession, mercantile business, or even pleasure, such as boating, cricketing, or shooting, intends to assert that he can pursue these respective objects with more success after he has taken a stimulant, then he may have an argument in favour of its use. If, however, it be admitted that during the active pursuits of the day a stimulation to the nervous system is injurious, but that after the fatigues are over the body must be recruited, and that a proportion of alcohol is beneficial, I have nothing to say against it, should experience speak in its favour. I repeat, however, that in the majority of instances where a man's reason for taking his wine or spirits is of no better kind than that he feels better for it, the reason is utterly valueless; indeed, it may generally be assumed that whilst his feelings are benumbed his organisation is being injured. The argument is no better in favour of the use of wine or other stimulant in disease—as, for example, that it must do good, since the patient craves for it. The question of the advantages attending the daily use of beer, wine, or spirits, although a difficult one to solve, is one which you cannot evade considering, since the health and welfare of families may depend upon your judicious decision. You may recommend wine with advantage to members of certain families having peculiar temperaments, whilst should you advise it for others you may unwittingly be sowing the seeds of ruin of mind, body, and estate.

The subject of the different temperaments of your patients and their mode of life is one which is well worth your study; but the matter I wish now to strongly enforce upon you is that you are as thoroughly to consider the propriety of the administration of alcohol as you would any drug in the Pharmacopœia. Endeavour, if you can, to erase from your minds that it is a proven fact that alcohol is a tonic or a necessary part of everyone's beverage. This is assumed by a large mass of people; and the meaning of the question which your patient puts to you when he says "What shall I drink?" is not "Shall I take a stimulant or leave it alone?" but "Shall I drink beer, wine, or spirits?" He often confesses that he is in a great difficulty; he finds none of them agree with him; but that he must take "something" appears as necessary as eating his daily bread; the alternative never having formed part of his calculation. I say it is assumed that a strength-giving property lies in these drinks—that just in proportion to a man's feeling of weakness so will he require one of them: in ordinary health he may only want his beer; but if ill, his wine; and if very ill, his spirits. Now this popular opinion is shared in, I am sorry to say, by many in the profession; if the patient is weak he wants "support," this term carrying too frequently with it the necessary idea of wine or spirits. I should be sorry to say that the doctor panders to the public taste, since he is too often already in accord with it; but the consequence of such agreement between patient and medical man resolves itself into this, that an extra stimulant is prescribed. You might ask to what complaints do I refer when I speak of this too common advice; but I need only repeat the word "patient," for it matters little what is the nature of the disease, since the reasons for the treatment are applicable to all complaints, and are founded on this simple proposition: all persons who are ill are weak; they have lost strength; they require it to be restored; alcohol is a supporter and a tonic, therefore alcohol is a remedy for all diseases. This is no parody, for I have heard the argument set forth in some such words; and practically it is adopted by many, for I constantly hear medical men say they give brandy to all their patients, for they always find them "low." Brandy indeed becomes with some as much a universal remedy as revalenta, chlorodyne, Morison's pills, or any other quack medicine. Moreover, it is a medicine of which the patients approve, assuming as they also do its supporting and strength-giving powers. You therefore cannot do better, if you fear no compunctions in converting your profession into a mere trade, to say to all your patients, after feeling their pulse, that they are very low—that you are sure they do not take enough; and order them several glasses of wine daily. Should they be exceedingly ill with some desperate organic complaint, then you must turn your remarks to the friends, and speak of the necessity of supporting the patient by giving him as much brandy as can be poured down his throat. By this method you are sure to give "satisfaction": for should the patient die without such treatment, you may have the credit of letting him slip "through your fingers"; whilst, if he die with it, you have done your best. If you kill a dozen patients with brandy, you need have no fear—"you have done your best." This, I say, would be a very comfortable and lucrative mode of practice.

It may very fairly be asked, If alcohol be so potent a remedy that it can supersede all drugs in so many several cases of disease, is not this a reason why brandy-treatment should be adopted with some consideration? The want of caution in its use is owing, I have no doubt, to its entering so frequently into the daily diet; and thus alcohol is not reckoned amongst the same class of agents as that of medicines. On the bed-cards in this hospital there is one column for the medicines, and another for the diet. Before filling up the one, we discuss the benefits of giving our patients a few drops of henbane or ether; and in the other column we often write down any number of ounces of brandy with very little thought of its effect. If alcohol were transferred to the medicine side of the card, we should be more likely to discuss its value in any given case in the same manner as we do the various drugs in the Pharmacopœia.

It would require a whole course of lectures to dwell upon the beneficial or baneful effects of alcohol in all forms of disease; and, therefore, I will simply state, as a result of my own experience, that like other drugs, it may be beneficial, useless, or harmful. I may remind you of what you yourselves have witnessed—that fevers will do well without this remedy. So wedded, however, are some to the idea of the absolute necessity of stimulants, that they have expressed almost incredulity when they have heard it stated that fevers will terminate favourably without them. Of course stimulants are often needed; but young persons with typhus and typhoid do far better, I believe, without them. That they make good recoveries on simple milk diet is a fact which my hospital cases prove, and which no arguments can gainsay; and, on the other hand, I have seen a marked improvement take place in some cases where a stimulus has been left off. It is also a fact that in bronchitis I have repeatedly seen improvement after stimulants have been omitted; and, as regards heart-disease, I am convinced that the amount of mischief done by stimulants is immense. In the case of fevers and bronchitis, the weak pulse is often but an indication of extreme capillary congestion, and a stimulus to the heart only aggravates the evil; and in the case of a diseased and weak heart, where repose is indicated, a constant stimulation by alcohol adds immensely to its trouble.

It causes me daily surprise to observe how the effects of stimulation are overlooked. Often have I been called to see a patient apparently dying, sometimes of a nervous disorder, at another time of a liver complaint, and at another of heart disease. He is lying in bed, where he (or she) has been for some time, and kept alive (as it is said) by brandy; the breath is abominably fetid; the heart's action is so rapid that it is impossible to say whether the organ is diseased or not; the patient refuses food, or if this be taken, it is rejected, and so he is plied with brandy to keep him alive; the body is, in fact, saturated with spirit, or its elements. My first remark on seeing such a case is, that a man cannot live on alcohol; he must take some food or he will die. The correctness of such common-sense remarks is admitted, but qualified with the statement that no solids can be taken, and that if stimulants be omitted it is feared the patient will sink. It is assumed that the constant administration of brandy is necessary for the temporary maintenance of life, and the idea never seems to to have been conceived that the stimulation of the heart causes the weak, fluttering pulse, and the stimulation of the stomach a subacute gastritis. Do you ask me what method I adopt? The simplest possible. I withdraw every drop of the stimulant, and in a few hours the irritated stomach is partly restored to its normal condition, the nervous excitement abates, the patient takes a little food, and begins to mend. Do you ask, again, whether I do not fear any frightful results from the sudden withdrawal of the stimulus? I say, not the least; I have no fear of the consequences. Not of delirium tremens.? Not in the least. This is a disease not induced by the withdrawal of stimulants, but, on the contrary, is produced by a recent debauch. For the production of delirium tremens the patient must have been such an habitual tippler as to have weakened his brain, and must then have had an overdose af the stimulant to set up the disease. There are no facts to show that the withdrawal of the accustomed drink is attended with any evil results, although I know that an imaginary fear of this kind leads to an erroneous and vicious method of treatment—the plying the patient with a stimulant during the violence of the attack, the effect of which is to prevent or prolong the cure. Rest and repose, with the avoidance of stimulation, is the treatment which the patient requires. The success of digitalis may be mentioned in corroboration of this view. I repeat that there are no facts to show that delirium tremens is produced by the withdrawal of stimulants, whilst it is a fact, as I could illustrate by many cases, that nothing but good results from its absolute discontinuance in the desperate cases to which I have alluded.

That many cases of disease of various kinds would do far better without stimulants I am perfectly confident. But lately I have seen the case of a gentleman, about sixty years of age, who passed through a most severe attack of pneumonia without the use of stimulants. He had been a tolerably free liver, and would not have been called a good subject; but having before me the case of another gentleman of the same age, who had just died of pneumonia, and who had taken a large quantity of brandy, I readily acquiesced in the patient's own view, that none should be given. It is very remarkable what extremes we have reached, and on how slight a scientific basis is founded the treatment of pneumonia. Not many years ago the antiphlogistic method was adopted, including bleeding, antimony, calomel, &c.; then came the "let alone" method; and now we have the brandy treatment. What the need of this can be with Professor Hughes Bennett's statistics before us, I do not comprehend. My own opinion is (but of course this is only an opinion), that in any given number of cases a larger majority would recover under the old antiphlogistic treatment than by the more modern method by brandy. As regards heart-disease, the utmost discrimination is required in the use of stimulants. There are cases where an undoubted benefit is produced by them ; but there are others, and these I have seen repeatedly, where alcohol has induced palpitation, fluttering, great distress, and constant sleepless nights, but where, on the other hand, the withdrawal of the spirit, and the substitution of a dose of digitalis or henbane, has been of the most essential service. The administration of a stimulus in the attempt to overcome disease, in lieu of good and well-tried remedies evinces the very worst form of medical scepticism with which I am acquainted.

It is not only in these severe cases of disease, but in lesser troubles, that your recommendation of stimulants may do incalculable mischief. You visit, for example, an ailing lady, and she details to you a number of troubles of a nervous and dyspeptic character. She is sitting in-doors all day, taking no exercise, living well, and consequently drifting into a weak and flabby condition. You place your hand on her pulse, and finding it feeble, condole with her on her state of health, assure her that she does not live well enough, and order her a few extra glasses of wine or a little brandy. You find that she grows no better for the advice; but perhaps you never reflect that you have been adding fuel to the fire. Knowing not what to do in the way of treatment, you order her out of town, and she immediately begins to improve. She goes to Brighton, rides on horseback or walks miles a day on the Parade, regains her appetite, craves less for stimulants, and her health is restored. If, on the contrary, you fail to remove her from her home, she goes on from bad to worse; she takes to her bed, eats less food, drinks more wine and brandy, until, having become one mass of fatty degeneration, life can hold no longer, and death ends the scene. This lady has been killed with kindness. This is no imaginary case; my mind's eye is carrying me to the bedside of more than one such instance. Do not then assume that alcohol is an equivalent to a tonic, and that it must be necessarily administered because your patient is weak. It may be that that very weakness is due to the long-continued pernicious effects of this same stimulant; indeed, as you have often heard me say in the out-patient room, if a man comes into our presence with a tottering gait, bloated face, and his nervous energy all gone, you may be quite sure that he has been taking "strengthening" things all his life.

I will say no more on the subject, as I do not wish to speak condemnatory of alcohol as a remedy, since it is one of the most powerful agents we possess to rouse the dormant nervous power. Moreover, I do not wish to speak too dogmatically of its ill-effects, being fully aware that there are many holding very distinguished positions in the profession whose opinions are not in accordance with those I have expressed. Were it not for this reason, I should have used still stronger language than I have done; for even firm convictions must be restrained when we know that an amount of contrary opinion can be arrayed against us. It is, nevertheless, the duty of everyone to express his own conviction when that is based on experience, and thus I shall ever feel bound to withstand the indiscriminate use of stimulants in disease.

Whatever may be thought of the remarks just made, there is one thing which I must insist upon—that is, when treating any malady, and the administration of alcohol is suggested to your mind, that you give the same grave consideration to its recommendation as you would to any other potent drug in the Pharmacopœia; not to sit down and give all your serious thoughts to the question of whether a grain of this or a grain of that drug should be ordered, perhaps twenty or thirty drops of ether, and then at haphazard order any loose number of ounces of brandy. You observe that I say nothing against the potency of alcohol in several states of disease; but I do speak strongly against its indiscriminate use without due consideration of its need or of its results. My arguments would equally apply did I find that opium or any other drug were indiscriminately used as a universal medicine. I should protest against the practice, whilst still possessing great faith in the virtue of the drug. If I can influence you to place alcohol in your list of drugs, so that you may administer it with the same caution as you do the several articles in the Pharmacopœia, then the object of these remarks will be fully answered.—*Lancet, April* 27, 1867, *p.* 505.

9.—AN ANALYTICAL EXAMINATION OF THE CAUSES OF GOUT.

By Dr. Charcot, Assistant-Professor at the Paris Faculty of Medicine.

After having briefly surveyed the history and geography of gout, we must now descend from this philosophical and comprehensive point of view to the minute examination of facts, in order to appreciate the conditions which, under various circumstances, give rise to that disease. In the course of this lecture, we shall seize the opportunity of calling your attention to some interesting particulars which, up to the present time, had remained almost entirely unknown in France.

I. *Individual Causes.*

1. *Spontaneous Development.*—It is certain that gout, in some cases, arises spontaneously. The fact is corroborated by the testimony of all the authors who have written on the subject; and in my own practice I have met with cases of this kind. There exist, therefore, in the constitution of certain individuals, conditions which favour the development of gout; and external circumstances only serve to bring them to light. We need scarcely be astonished at this; for an excess in the production, or a deficiency in the elimination, of uric acid appears to lie at the root of the gouty diathesis. But in the normal state a small proportion of uric acid already exists in the blood: let its amount be increased, and the morbid process will at once begin.

2. *Hereditary Predisposition.*—The definition of gout, as given by modern writers, invariably comprehends the notion of hereditary transmissibility. Hospital Physicians have already ascertained its existence; and in town practice it is still more prominently brought forward. A few figures will better impress the fact upon your minds. The hereditary taint has been noticed

By Scudamore,	in 523	cases of gout,	309	times.
,, Patissier,	,, 80	,,	34	,,
,, Garrod,	,, 100	,,	50	,,

Hereditary gout is often premature; it frequently makes its appearance before the age of 30 or 35, the usual period for the spontaneous manifestation of the disease. It often exhibits itself at the same age among all the members of certain families. Garrod informs us that in one of the most illustrious houses of England the eldest son is invariably affected with gout on succeeding to the title; and this singular inheritance has been entailed in the family for the last four hundred years.

3. *Sex.*—The influence of sex on the production of gout is evident. It cannot be denied that females enjoy a peculiar immunity in this respect. Out of the eighty cases collected by Patissier, seventy-eight were observed in men, and only two in women. The change of life is the period at which the disorder chiefly exhibits itself in female patients—a remark which had not escaped the sagacity of Hippocrates.

We discover here an essential difference between gout and chronic rheumatism. This rule suffers, no doubt, a few exceptions, and some ladies are affected with gout early in life; but in such cases we almost invariably recognise an hereditary taint.

It must be added that the low asthenic forms of the disease predominate in female subjects.

4. *Age.*—The classical time of life for gout extends, according to Scudamore, from 30 to 35. It is seldom met with before 20 or after 60. Garrod, however, has seen it once in a boy 9 years old, and once in a lad of 17. He has also observed a few cases in which the first attack occurred in men of 60.

Rheumatism, on the contrary, is met with at an earlier age, and usually makes its first appearance before the patient is 35.

5. *Constitution.*—Attempts have often been made to establish the characteristics of a peculiar constitution which predisposes the subject to this disease; but experience has shown that gout spares no constitution whatever, and assails weak, debilitated patients as well as powerful, athletic men. The type of the disease corresponds, however, to the general condition of the system. The sthenic or acute form is principally observed in strong plethoric subjects; the low asthenic form in women and invalids.

II. Hygienic Causes.

From the causes of gout which are inherent to the patient himself, let us now turn to those which result from the conditions in which he lives. Let us examine his habits, and more especially his diet; we shall discover facts of the highest importance in prosecuting this study.

1. *Climate.*—Medical geography has already taught us that gout belongs almost exclusively to the temperate zones of the globe, and flies from tropical regions; but, although entirely unknown in Brazil, in Africa, and under the line, it occasionally affects Europeans, who have brought into hot climates the habits of northern countries. For this reason the English in India are sometimes troubled with its symptoms, though less frequently than in their own native country.

2. *Full Diet, and Want of Exercise.*—It has at all times been acknowledged that an easy life and high feeding—two conditions not unfrequently combined in the same individual—constitute a strong predisposition to gout; it predominates, in consequence, among the higher classes of society, and is less prevalent in the lower ranks. In familiar style, gout is often said to be the result of a surplus of receipts. We shall soon see that facts cannot always be so easily interpreted: but it is quite certain that an excess of animal food promotes the development of this complaint, and that voracious eaters are frequently numbered among its tributaries.

3. *Influence of the Nervous System.*—The influence of cerebral activity cannot in this case be denied. Intellectual labour, strong emotions, great mental exertions, have always been numbered among the causes of gout. Sydenham, who was himself a martyr to this affection, attempts to console himself

by reflecting that "*divites interemit plures quam pauperes, sapientes quam fatuos.*" The chief politicians of England have been numbered among its victims. We need only mention the two Pitts, father and son. The Earl of Chatham was by no means a votary of Bacchus. It is true that the same can hardly be said of his illustrious son, who never addressed the House of Commons without having previously inflamed his eloquence by freely resorting to the bottle.

4. *Venereal Excesses.*—The depression of the nervous system which results from sexual indulgence creates no doubt a strong predisposition to gout. But a natural explanation here suggests itself. Looseness of morals in general is closely allied to intemperance, which in this case probably plays the chief part in the production of disease.

5. We have now to examine the action of two of the most important causes of gout—I allude to *fermented liquors* and *lead-poisoning.*

Such is the influence of fermented liquors that, according to Garrod, man deprived of these drinks would never have known gout. The action of lead-poisoning is far less extensive, but, as regards the theory of the disease, it is a fact of the utmost importance.

We will now proceed to investigate these two points.

Fermented Liquors.—A broad line of distinction must be drawn between spirits (rum, brandy, whisky, gin, etc.), which contain from 40 to 70 per cent. of alcohol, and ordinary fermented drinks (wine, beer, cider), the alcoholic strength of which varies from 4 to 20 per cent. At first sight it seems natural to suppose that the more potent liquors are by far the most conducive to gout. But you will perhaps be astonished to find that the use of ardent spirits does not appear to be connected in any degree with the production of this disease. In fact, gout is far from being widely diffused among the dram-drinking populations of Europe. It is scarcely ever met with in Sweden, where *alcoholism* is so frequent, according to Professor Magnus Huss. The same is the case in Denmark, Russia, and Poland. In Scotland and Ireland gout is seldom observed among the lower classes. At Edinburgh Bennett and Christison have scarcely met with more than one or two cases in their hospital practice. Now the only strong drink in general use in those two countries is whisky.

In London, on the contrary, gout is exceedingly frequent among workpeople, and is constantly observed in Hospitals. Now the only important difference which exists in this respect between the northern and the southern parts of Great Britain is the enormous quantity of *strong beer* absorbed by the inhabitants of the metropolis, under the various names of *ale, stout,* and *porter*. The extraordinary influence of these drinks is acknowledged by all English medical writers. Scudamore informs us that since the use of porter has been universally adopted in London, gout has become infinitely more frequent among the labouring classes. Watson, Budd, and Todd corroborate this assertion. According to the last-mentioned author, most of those who usually drink beer, and especially porter, become, sooner or later, victims of gout.

An interesting instance of this is recorded by Budd, whose words we shall here beg leave to quote:—"There is a body of men employed on the Thames whose occupation it is to raise ballast from the bottom of the river. As this can only be done when the tide is ebbing, their hours of labour are regulated by that circumstance, and vary through every period of night and day. They work under great exposure to inclemencies of weather: their occupation requires great bodily exertion. . . In consideration of this, their allowance of liquor is very large: each man drinks from two to three gallons of porter daily, and generally a considerable quantity of spirits besides. This immoderate consumption of liquors forms the only exception, as far as relates to food, which these men offer to the general habits of the lower classes in London. Gout is remarkably frequent among them, and although not a numerous body, many of them are every year admitted to the Seaman's Hospital ship affected with that disease." These poor labourers are therefore admitted for once to share in the privileges of the peers of the realm, yet they are almost all derived from the peasantry of Ireland, and can scarcely, therefore, inherit a disposition to gout.

Garrod has also arrived, on his side, at similar conclusions. He states that persons employed in breweries are frequently affected with gout; and the abuse of malt liquors, especially porter, is the only plausible explanation of this morbid tendency.

And yet the various kinds of beer are far from containing any large amount of alcohol. Mulder states that Scotch ale contains 8⅝ per cent., and porter only 5 per cent. This proportion is below that of our French wines, and not superior to that which exists in German beer—a liquor the effects of which are entirely different, notwithstanding the enormous quantity absorbed by our neighbours across the Rhine.

You therefore see, that *à priori* reasoning cannot be brought to bear on this subject, and that the influence of fermented liquors on the production of gout is entirely independent of the amount of alcohol which they may happen to contain. Other conditions, our knowledge of which is hitherto very imperfect, appear to take the lead, and it is only from experience that we can learn the real state of things.

Let us now examine the action of wine. The highest rank, as far as our subject is concerned, evidently belongs to the wines (port, sherry, Madeira, Marsala) which are so generally preferred in England by all classes of society. They all contain a large proportion of alcohol, which varies from 17 to 20 per cent.

Light wines (Rhine, Moselle, champagne, claret) are far from exerting a similar influence.

But the same cannot be said of Burgundy, which, however, does not contain more spirit than the qualities we have just named. Gout is contained in every glass of red Hermitage and Burgundy, especially the latter, if Scudamore is to be trusted.

Cider itself, an apparently inoffensive beverage, is said to favour the development of this disorder. According to Garrod, this injurious property does not belong to *hard cider*, and only exists in the new unfermented liquid.

Having, as we believe, sufficiently illustrated the connexion between gout and fermented liquors, let us now turn to a different subject.

Lead Poisoning.—Garrod had ascertained some time ago that out of eighty-one gouty patients admitted into his wards no fewer than sixteen were painters and plumbers, and ulterior investigations have corroborated this singular result. The introduction of *lead* into the system must therefore be placed in future among the causes of gout.

The fact having been established, documents in its favour came pouring in from all side. Among the authors who had noticed the coincidence before Garrod we will quote Musgrave, who saw gout appear immediately after *colica pictonum;* Falconer, who notes the same fact; Parry, who shows that gout frequently exists in cases of paralysis from lead poisoning; and lastly Todd, who mentions several cases of gout observed under similar circumstances.

Since the appearance of Garrod's excellent work, several English authors have published observations of this kind ; we will only mention Burrows and Begbie. But the habits of English workmen, which have just been discussed, exert, in all probability, a powerful influence in such cases. In France, where lead colic is so frequent, why is gout so seldom met with among the lower classes ?

A few gouty patients exist, however, among those who have undergone the process of lead poisoning. I have been so fortunate as to observe the coincidence myself in on one most remarkable case, and Dr. Bucquoy has just met with another at La Charité.

The cause of this singular connexion between two very different disorders still remains obscure. Garrod has discovered that lead poisoning produces an undue accumulation of uric acid in the blood, especially in an advanced stage, where paralysis already exists. The fact has been ascertained in patients who were not labouring under gout, and who do not seem to have been affected with Bright's disease. On examining the urine the proportion of uric acid was found to have considerably diminished, but we are not informed whether there existed any degree of albuminuria. Garrod is not able to decide whether there is an excess in the production of uric acid or an insufficient elimination. He leans towards the latter hypothesis, and relies upon the following experiment.

After having analysed every day, during a certain lapse of time, the urine passed by several patients affected with various diseases not connected with the urinary organs, he gave them medicinal doses of acetate of lead. Under the influence of this preparation, the excretion of uric acid was seen gradually to decrease.

If, therefore, the introduction of lead into the system calls forth the manifestation of gout, its action seems principally to bear upon the kidneys. It paralyses their functions as far as the excretion of uric acid is concerned. But can this single cause give rise to the gouty diathesis? It may do so in a few insulated cases; but if there exist other predispositions acting in the same direction, the effects of lead poisoning will appear in their full light.

III. Exciting Causes.

Although incapable of creating gout by themselves, the conditions which we are now going to enumerate are sufficiently powerful to provoke attacks of the disease when it has already taken possession of the system.

1. *Alcoholic Liquors.*—In gouty subjects certain wines (port, champagne, &c.), even if taken in small quantities, will determine an attack of gout, which is sometimes limited to the great toe. According to Garrod, whenever a few glasses of wine produce inflammation of the joints, that inflammation is invariably connected with gout.

2. Indigestion and all other derangements of the stomach act in the same manner.

3. Cold and damp, and suppressed perspiration, produce similar effects.

4. Immoderate intellectual exertions exert on the production of gout an influence to which we have already called your attention.

5. Surgical injuries, operations, fractures, act in the same way. I have myself seen a wound produce simultaneously trismus and gout.

6. Debilitating causes, such as loss of blood, protracted illness, &c., &c., have also a marked influence in the production of gouty symptoms. This fact deserves notice, gout having often been described as a malady peculiar to plethoric subjects; but Todd has amply proved that it is equally severe in debilitated patients.—*Medical Times and Gazette, May* 4, 1867, *p.* 463.

DISEASES OF THE NERVOUS SYSTEM.

10.—ON THE USE OF STRYCHNINE IN EPILEPSY.

By Walter Tyrrell, Esq., Malvern.

[Mr. Tyrrell published a paper on this subject in May, 1867.]

I stated at the end of my former paper, my belief that in "strychnine we possess a drug which will always control the excitability of the medulla oblongata, and restrain the attacks of convulsion." This opinion will, I think, be found to be remarkably strengthened by a perusal of the following cases. One most important fact is to be gathered from them—viz., that large doses of the drug must be given to produce the favourable results. In some of the cases the doses may appear formidable, but I feel confident that with care and watchfulness no ill effects need follow their administration to the epileptic. In such cases the system appears to lose its susceptibility; and the drug even in large doses produces none of the ordinary signs of disagreement. In no case have I seen it produce any mischievous excitement or irritation; and I may state that in one very severe case, still under treatment, I have carried the dose as high as one-fifth of a grain, taken twice daily, and this continued for nearly three weeks together, not only without its producing the slightest sign of irritation, but on the contrary the most marked diminution in the frequency and violence of the attacks. The following case, although the attacks had but recently come on, is interesting as showing how rapidly the beneficial effect of strychnine is often gained, no attacks having supervened after two doses (each of one-twelfth of a grain) had been taken.

H. R., aged 29, has been of late years much exposed to heat in China, Singapore, and Japan; had congestion of the liver in March, 1865; was invalided home in June, 1865; since which time he has been living at home, under treatment for enlargement of the liver, using iodide of potassium and iodine ointment locally. On May 22 of this year—a very cold snowy day—he imprudently stayed out all day fishing, and at dinner that evening was seized with a violent epileptic fit, accompanied with great convulsion; this was followed by other attacks at the following intervals:—May 25, three fits, at intervals of one hour and a half; May 30, a fit in the evening; June 1, two fits, with six hours' interval; June 2, one fit in the evening. On June 5 he arrived in Malvern, and I prescribed for him one-twelfth of a grain of strychnia twice daily, allowing him to continue his potash in rather increased quantity. On the morning of the 6th he had three fits, during the first of which I was present; they were very convulsive, and produced an extremely prostrating effect on his mind—so much so, that, even after the ordinary stupor had passed off, he was unable to answer the simplest question without consideration and great hesitation. It is needless to give a daily report of this case. I increased the dose of the strychnine to one-eighth of a grain; he had no further attacks; and his return to health, both bodily and mental, although gradual, was most perfect. He is now at the seaside, and may be considered to all intents and purposes convalescent. In this case it was curious to observe how the inclination to an attack (which occurred several times during the early treatment of the case) yielded at once to a slight increase in the strength of the dose. I may say that in this case I found the use of ice to the nape very useful, insuring quiet sleep, and also allaying a frequent tendency to irritability.

In the following case, where the attacks were dependent on menstrual irregularity, the utility of combining the strychnine with remedies directed to the removal of the exciting cause will be apparent.

L. A., aged 17, a not unhealthy-looking girl, has never menstruated properly; has been subject to epilepsy for four years, the interval never been longer than one week; the attacks vary in intensity, a slight one being sooner followed by others. In this case I commenced with one-sixteenth of a grain of strychnia twice daily, gradually carried up to the tenth, at the same time giving her aloes and myrrh and assafœtida in pills twice daily. In this case a perfect immunity from attacks commenced with the treatment, and has continued up to the present day, a period of nearly three months. Although the menstrual irregularity has not entirely ceased, it is very much ameliorated. I used also in this case the cold affusion to the nape, coupling it at times with the application of warmth to the feet. This case, although not severe, is a type of a very prevalent form of the disorder, and shows how amenable such cases are to treatment. In another case somewhat similar, which is still under treatment, I have the greatest benefit from the use of the bromide of potassium in combination with strychnia.

E. H., aged 14, a fair girl, partially paralysed on the left side. When two years old had what was called brain fever, during which she was insensible for a length of time; recovered, but had a return about two years ago. Since the first attack she has been subject to continued attacks of *petit mal*, sometimes five and six in the day. She turns slightly to the right; is slightly convulsed; sometimes is partially conscious during them, and tries to talk; sometimes she bites her tongue; her manner is silly, being fond of repeating lines of poetry, for which her memory is good. She has slight tenderness on pressure over the upper cervical vertebræ, and on percussing the atlas with the finger points she complains of pain at the epigastrium. The attacks sometimes come on during sleep. I will give here an extract from the dairy kept by the parents. The patient came under my care in May, and I prescribed for her:—℞. Tinct. nucis vomicæ ʒiij., syr. aurantii ℥j. M., cap. ʒj. bis in die ex aquâ.

The following is a dairy from May 29 up to the stoppage of the attacks:—

May 29. Four fits in the day; two in the night.

30th. No fits in the day, but eight in the night, two of them being severe.

31st. One fit in the morning; eight again at night, but less severe.

June 1. No fit in the day; four at night.
2nd. No fit in the day; five at night.
3rd. No fit in the day; four at night.
4th. No fit in the day; three at night.
5th. No fit in the day; three slight ones at night.
6th. No fit in the day, and if any at night, very slight.
7th. No fit in the day; only one observed at night.
8th. No fits day or night.
9th. No fits day or night.
10th. No fit.
11th. No fit.
12th. No fit.

And so on. Since this date she has continued almost entirely free from attacks, but few having occured, and those of an altered and much slighter character, which yield readily to a slightly increased dose of the strychnine.

Although the following case is still under treatment, yet I think a slight sketch of it cannot fail to be interesting as exhibiting the effects of strychnine in very severe convulsive epilepsy, and as also showing what large doses of the drug may be given with impunity. This patient, who has suffered for some years, is one of the severest cases of the disorder I have ever seen, I commenced treating him on the 15th June last, and as the case is still under treatment, I will merely give a comparative table of the number of his attacks during May of the present year and July. During the former month he was under no treatment of any kind. During July he was taking strychnine in the doses appended to the table. The attacks, which occurred almost exclusively at night, were most violently convulsive. They were much influenced by atmospheric changes, heavy thundery weather invariably increasing both their number and severity. Thus July would, under ordinary circumstances, be his most unfavourable month. In addition to the strychnine during part of the month, he was using cold affusion to the nape, and ice to the occiput during the night.

No Treatment.

May 1, 1867 . .	2 fits	May 17, 1867 . .	1 fit
,, 2, ,, . .	1	,, 18, ,, . .	0
,, 3, ,, . .	2	,, 19, ,, . .	0
,, 4, ,, . .	3	,, 20, ,, . .	0
,, 5, ,, . .	2	,, 21, ,, . .	1
,, 6, ,, . .	3	,, 22, ,, . .	0
,, 7, ,, . .	2	,, 23, ,, . .	2
,, 8, ,, . .	2	,, 24, ,, . .	2
,, 9, ,, . .	1	,, 25, ,, . .	3
,, 10, ,, . .	0	,, 26, ,, . .	2
,, 11, ,, . .	2	,, 27, ,, . .	2
,, 12, ,, . .	2	,, 28, ,, . .	1
,, 13, ,, . .	4	,, 29, ,, . .	2
,, 14, ,, . .	3	,, 30, ,, . .	0
,, 15, ,, . .	3	,, 31, ,, . .	0
,, 16, ,, . .	3		

Under Strychnine.

	Fits.			Fits.	
July 1, 1867	0	$\frac{1}{8}$ gr.	July 16, 1867	3	
,, 2, ,,	1	very slight	,, 17, ,,	0	$\frac{1}{5}$ gr.
,, 3, ,,	1	very slight, no con.	,, 18, ,,	0	
			,, 19, ,,	0	
,, 4, ,,	0		,, 20, ,,	0	
,, 5, ,,	0		,, 21, ,,	0	
,, 6, ,,	0		,, 22, ,,	0	
,, 7, ,,	1		,, 23, ,,	0	
,, 8, ,,	2		,, 24, ,,	0	
,, 9, ,,	0	$\frac{1}{6}$ gr.	,, 25, ,,	0	
,, 10, ,,	0		,, 26, ,,	0	
,, 11, ,,	0		,, 27, ,,	1	
,, 12, ,,	0		,, 28, ,,	1	
,, 13, ,,	0		,, 29, ,,	0	
,, 14, ,,	1	$\frac{1}{12}$ gr.	,, 30, ,,	0	
,, 15, ,,	0		,, 31, ,,	0	

On July 14, owing to a misunderstanding, he only had $\frac{1}{12}$ of a grain instead of $\frac{1}{6}$th. It will be seen that four fits followed in rapid succession. I think this table shows the power which strychnine possesses in restraining the epileptic attacks. I may add that, although so remarkably lessened in number, they were not at all increased in severity, but, on the contrary, were less convulsive. The above table gives the following results :—

May.		July.	
No. of attacks.	Nights free.	No. of attacks.	Nights free.
51	7	11	23

It will be observed that during the latter half of the month the dose of strychnine was as high as one-fifth of a grain, taken twice daily, and this without its producing the slightest sign of excitement or irritation. In combination with the strychnine the patient is taking the infusion of digitalis.

In conclusion, I would reiterate the summing up of my last paper—"That in strychnine we possess a drug which will *always* control the excitability of the medulla oblongata and prevent convulsions, but that to cure the disease we must also remove the exciting cause.—*Medical Times and Gazette, Aug.* 24, 1867, *p.* 201.

11.—ON A CASE OF TRAUMATIC TETANUS SUCCESSFULLY TREATED WITH THE ORDEAL BEAN OF CALABAR.

By Dr. A. Campbell, Navenby, Lincolnshire.

E. L., a delicate-looking girl of 12 years, was chopping meat with a chopping-knife on the 1st of January, 1867, when she cut the dorsal aspect of her left thumb, near the articulation of the metacarpal bone with the trapeza. The wound healed slowly by granulations, leaving an ugly elevated cicatrix. On the 14th of January, when the wound had nearly cicatrised, she felt a stiffness about the jaws which passed off on the application of a mustard poultice and a little medicine. She now continued in her usual state of health till the 1st of March, when she again experienced a sensation of stiffness of the muscles of the lower jaw, and slight twitchings of the muscles of the face. On the 2nd she was worse, and had a difficulty in opening her mouth. On the 3rd she had greater difficulty in opening her mouth, and had occasionally severe spasms in her arms and legs. She now continued getting gradually worse, and on the 8th, when Mr. Smith, my partner, was called to see her, she presented all the appearance of one who had trismus. The jaws were firmly locked, and the lower jaw was drawn forward, so that the teeth of the lower jaw were about a quarter of an inch in front of the teeth of the upper jaw, which enabled him to get fluids easily into her mouth. Her body and limbs were perfectly rigid, and the peculiar sardonic grin was very marked. The slightest noise, or moving her in the least, brought on the most violent spasms. She found comfort in lying on her stomach, with her head and shoulders over the edge of the bed. When the spasms came on she tried hard to get hold of anything in the way, so as to prevent the tendency in the direction of opisthotonos. A dose of castor oil, with turpentine, was now prescribed, and a mixture containing chloroform to be taken every three hours.

On the 10th the bowels had not been moved. To have an enema, which relieved her bowels; and to take a grain of opium at bedtime.

She continued much the same till the 13th, when I saw her for the first time (two weeks from her attack), when she appeared the most pitiable object I have ever seen. She seemed all of apiece, the muscles being as rigid as a board; the slightest noise produced severe opisthotonos, the body being bent into an arch, the girl resting upon the back part of her head and heels. The muscles of the back were so powerfully contracted as to cause angular, or Pott's, curvature of the spine. The scapulæ were drawn backwards forcibly, fixing the chest so that respiration was performed with difficulty. I ordered her an enema of castor oil and turpentine, and to have half a grain of opium every three hours.

I then consulted with my partner as to the advisability of trying the Calabar bean, so highly recommended and so successfully used by my respected teacher, Dr. E. Watson, of Glasgow. We agreed to give her the benefit of it (although my partner was sceptical about new drugs). Not having the tincture recommended by Dr. Watson at hand, I began on the 17th by giving her five drops of a solution of the extract in white wine (twelve grains of the extract to the ounce of wine, five drops of which, therefore, contained about one-eighth of a grain) every half hour.

I may mention that the girl lived four miles from my residence, so that it was impossible for me to see her so often as I could have wished. However, her stepmother, who is a most attentive nurse, and to whom I explained the physiological effects I wished to produce, was most assiduous in her attention. She began with five drops about eleven a.m. of the 17th, and continued it every half hour.

On the morning of the 18th I found her about the same, although the nurse thought the spasms not quite so violent.

Pupils widely dilated; pulse 110. I now ordered eight drops every hour. At eleven p.m. (by which time she had taken about six grains, in thirty-six hours) she became half comatose; pupils considerably contracted; breathing more easy; no tendency to opisthotonos. The muscles of the extremities became quite relaxed; and she had a feeling of sinking, as if about to die. The nurse, being aware of the physiological effects anticipated, at once stopped the bean, and gave her two teaspoonfuls of brandy every half-hour.

At one a.m. on the 19th she had a natural motion of the bowels. She now became less distressed, and fell into a quiet sleep of three hours' duration—the first she had had since her illness. At the time of my visit she was very much depressed; pulse 120, fuller than it had been; pupils slightly contracted; breathing easier; muscles of the arms and legs quite flaccid, although unable to move them. When moved, she had still severe spasms of the muscles of the back; I was, however, pleased to find that the medicine had so far produced its great work. I ceased giving the bean for a time, and ordered her beef-tea and brandy *ad libitum*.

March 20th. I found her decidedly better. Pulse 110, soft and regular; she has more power over her arms and legs; can open her mouth a very little. Spasms in the back still violent when moved, but not otherwise. I now ordered the bean in quarter-grain doses, in pill, every two hours.

21st. Slept a little during the night. Can open her mouth so as to admit the edge of a teaspoon; speaks more distinctly; says she feels hungry. To have an egg beat up in brandy, and to take a dose of castor oil.

22nd. She slept during the night, and did not take her pills regularly. To have an enema.

She continued gradually improving till the 26th, when she could open her mouth freely; eats and sleeps well; bowels still constipated; sits up a little when propped in bed. No spasms of the extremities; but when moved still complains of great pain in her back. The muscles of her back very rigid.

27th. I ordered her half-grain doses every two hours, so as to counteract the violent spasms of the back.

28th. I found her not so well; less power over her extremities; spasms of the back not so painful. She has a nasty cough, which made me feel anxious. Stopped the bean; and to have nourishment and wine, with five grains of Dover's powder every four hours.

30th. Cough less troublesome; can move her limbs freely; pulse stronger. Has had no bean since the 28th.

By the 1st of April she was so well that, the weather being fine, she was taken out in a perambulator. Still considerable rigidity of the muscles of the back. I ordered chloroform and laudanum liniment.

2nd. To have half-grain doses of the bean three times a day.

6th. Can stand upon her feet, although she feels very shaky, and grasps at anything in her way so as to support her back. To have twenty drops of tincture of muriate of iron three times a-day in water. Takes her food with relish; bowels now regular, &c. The muscles of the back still rigid—more so on the right side of the spine than on the left. When moved suddenly, or when she attempts to walk, she has considerable pain in her back.

12th. Improves slowly; pain not so great in her back when she sits up or stands; muscles of the back softer; still considerable curvature of the spine. To continue the iron, and the pills to be left off

20th. The muscles of the back much softer, but painful when pressed upon; the spine gradually assuming its original form; feels the muscles of her legs weak when she stands upon them; otherwise quite well, and evidently gaining flesh. I now consider her cured so far as the tetanus is concerned; but, as she still requires great care and good nursing beyond the power of her parents' means to bestow upon her, I have recommended them to remove her to the Convalescent Hospital at Lincoln.

The facts of the case speak for themselves, and I publish an account of it in the hope that others may be induced to give the drug a fair trial in similar cases. There can be no doubt that it has a wonderful effect in producing relaxation of the tetanically contracted muscles. The muscles of the back, being the most powerfully contracted, were of necessity the most difficult to overcome. The pupils were never contracted to a point, although upon two or three occasions they were perceptibly so. Another physiological effect I am inclined to believe it produces (which has been already hinted at by Dr. Watson) is catharsis (see Lancet, March 2nd). It is satisfactory to think that the cases which have been successfully treated by the bean enable us to look forward with more confidence than hitherto in treating that terrible disease. In future, I would be careful, after having produced a decided effect upon the system, not to carry the effect of the bean too far; for by doing so, in order to overcome the more obstinate muscles, such as those of the back, one might paralyse the already relaxed muscles, and so retard convalescence.—*Lancet, August* 10, 1867, *p.* 157.

12.—THE INFLUENCE OF EXTREME COLD ON THE NERVOUS FUNCTIONS.

By Dr. B. W. Richardson, F.R.S., Senior Physician to the Royal Infirmary for Diseases of the Chest.

The phenomena observed on freezing the animal tissues, depend essentially on a change in the condition of the water of the body. By the cold the water is transferred from a liquid to a solid state. Nerve tissue contains eighty-five per cent. of water. Make this water solid, and we get all the phenomena of negation demonstrated—a sort of temporary death. These phenomena may be produced also by other means. Heat is capable of bringing about a similar state. Abstract the water, dessicate the tissue by any means, and the result is the same. In illustration of this, a solution of brain matter was placed on a piece of glass, and on the spray being played upon the opposite side of the glass, a visible change was brought about. This was owing to the solidification of the water, and the separation of the albumen. The same appearance was produced by dropping on the matter some absolute alcohol. This result was brought about by the great affinity of the alcohol for water, depriving the brain matter of this substance. Heat was next applied by means of a spirit lamp. This agency brings into play similar sets of phenomena.

Thus, by congelation on the one side, and coagulation on the other, we get exactly the same phenomena, the lecturer then went on to refer to some things which are more subtle in their action than these. But these more subtle agents are no less definite in their effects.

Take, for instance, shock, which suddenly produces the phenomena of negation, seen in freezing the nervous centres. Again, a blood change is equally subtle in its operation, and the same may be said of pressure on the brain, which exerts a very definite influence. The lecturer conjectured that probably the effects of certain sounds and odours on the brain, might be explained in a similar manner, and so of certain sights which are known to produce the same phenomena. Moreover, loss of blood leads to the same change—the abstraction of the watery part of the nerve tissue. This leads to a conjecture that some of the phenomena of the collapse stage of cholera may depend on the abstraction of water, and the crystallization of the nerve matter in the body.

One of the most striking things in all these experiments on animals is the local independency of the several portions of the nervous system. Every part seems to have its own centre, so we can isolate many parts of the cerebrum. We may act on one hemisphere or both, or on the cerebellum, or on the medulla, or spinal cord. But we do not thereby necessarily interfere with the functions of the other parts of the nervous system. This fact explains many things in connection with, or in elucidation of, brain disease. There is, nevertheless, to be noticed a kind of balance of one part by another. In reference to this, it is clear that one hemisphere balances another. The corpora striata balance, and are balanced by, the cerebellum. The medulla is thus in the same relation with some obscure point not yet fully made out. One part of the medulla balances a part of the spinal cord connected with respiration. The medulla presides over the muscles used in inspiration; the part of the cord balanced by it presides over those of expiration. If we freeze the medulla we stop the breathing, but we stop it by arresting the inspiration. In the opposite case, that of freezing the segment of the cord balanced by the medulla, we stop the breathing by arresting the expiratory act. Thus, in the nervous functions there is a balance of opposite powers, just as in the muscular system there is a balance of flexors and extensors. This is of considerable import in a pathological point of view. In diseases in which there is a tendency to fall forwards for instance, this propensity may indicate disease of the corpora striata; but it is to be remembered that the disease may be negative while the symptoms only show over-action of the balancing part. The disease may be the result of negation of the function of the corpora striata, while the phenomena of the disease, the symptoms, are those of excessive or unregulated action of the cerebellum. Psychologically, these experiments are also interesting. This is, perhaps, illustrated in the act of going to sleep in the upright position. Sleep seems to begin in the visual organs. As the anterior part of the brain first loses its power, we see the tendency to nod—to fall forwards—caused by that part becoming inactive, while the posterior parts retain their function of propelling. The same may perhaps be said of the shock which sometimes results from looking down great heights. This shock paralyses the anterior or thinking part, and the cerebellum retains its power, and so gives an impulse forward. This question might be put to the test by sportsmen. A bird shot flying will fall at once, rolling over and over *forwards*, while another turns over many times backwards, in its descent. Dr. Richardson believes that in one case the bird will invariably be found shot through the cerebrum, and in the other through the cerebellum, and he suggested that this fact should be noticed by such of his hearers as had the opportunity of joining in the sports of the field.

In diseases, exactly the same reasoning applied. An instance of suicide by hanging was mentioned. The man, however, had on, at the time of his attempt, a soldier's stock, which, by its compression on the jugulars, caused extreme pressure on the brain, the ventricles after death being found full of effused liquid. He was cut down alive, and immediately ran forwards several yards to a wall; afterwards he had the same symptoms. A man at Newcastle-under-Lyne lost a portion of the bone of the skull, and on pressure on the brain, it was noticed that he pitched *forwards*. It was also observed that in certain states of the weather a similar tendency was evinced, probably when the atmospheric pressure was increased.

In therapeutics, the experiments may be of great value. First of all, they may indicate the mode of action of alcohol. This fluid abstracts water from all the tissues, and most of all from the brain. But there is along with this a liberation of heat, and then a loss of heat, as the alcohol passes away with the water it has abstracted. Thus we have direct loss of water from the brain-tissue, and loss of heat from the whole body.

By a subtler action, ether and chloroform modify the relations of water and albumen. It was thought, years ago, that chloroform suspended oxidation. The idea arose with Snow, but it was not clearly shown to be fact. Some have thought that the chloroform was absorbed and picked up caloric in the system, and by expansion in the closed cavity of the skull, caused pressure on the brain. This idea was suggested by finding that if the skull be opened while an animal is under the influence of the drug, the brain does protrude. But it appears that some more direct change takes place. Dr. Richardson had placed animals in a rarified atmosphere, and found the effects of chloroform precisely the same.

The effect of nitrite of amyle, more subtile and more remarkable still, was next adduced in support of this view. Then was adduced amylic alcohol. This suspends all nerve function, but the animal, though apparently dead, will afterwards recover. A guinea pig, to which this substance had been given, was then passed round, and appeared quite lifeless. The lecturer stated that it would recover the next day. On recovery from this poison, symptoms like strychnine poisoning are observed. It is a spinal poison.

Antiphlogistics.—The mode in which these may act was next referred to. The familiar experiment of the loss of heat on a solution of salt in water, was shown by the thermometer. Then a frog was injected with a solution of chloride of ammonium. This operation produced artificial cataract. The same effect was next produced on another frog by freezing the eyeball with the ether spray. In each case recovery took place.

The question was now propounded, can cold be applied for the cure of inflammatory symptoms? The spray has been used to subdue acute mania. Cold below zero, for half an hour at a time, to the anterior part of the skull, has been found quite safe. It has been followed by quiet sleep, and by no extreme reaction. In chorea the same agent has been used.

In recovery from rheumatic affections, in which there is excessive vascularity of the membranes of the cord, it has been used in France, and Dr. Richardson looked upon it as likely to cure.

In ganglion, formed on a nerve, it has been successfully employed to stop pain and motion. This led Dr. Richardson to try it in tetanic action. To show how far he had pursued this, he injected a frog with 1-45th part of a grain of strychnine, and placed it under a bell glass on the table. For two or three minutes it tried to escape, and seemed lively enough. Then tetanic rigidity came on. When the effect of the poison had had become so fully developed that the least touch caused violent convulsions, the cerebrum-spinal cord were frozen by the ether spray, and the effect of the strychnine was completely subdued. A frog does not eliminate rapidly, but possibly an animal with more effective secreting power might be kept frozen until the poison has passed away.

Could we then in tetanus keep a patient frozen until the tetanic poison could all be eliminated? If indeed that disease depend on a poison at all, and may not rather consist in some molecular change, such as may be caused by a shock or a blow: —if we had the hope, there is a great practical difficulty. Over the spinal cord there is so thick a layer of non-conducting tissue that no effect can be produced. In experiments with a delicate thermometer inserted in a portion of the spine, the spray being directed on the outside of the muscles, Dr. Richardson has not been able to make a difference of one degree. Though he would not assert that in the living body some effect might not be *indirectly* produced through the skin, or its vascular and nervous system.

In a warm-blooded animal even actual freezing of living structure may be induced without any stage of pre-action, provided always that the heat be abstracted with sufficient energy and rapidity. Let me demonstrate this on my own body.

We have here for our spray apparatus a very light hydrocarbon fluid, called rhigolene. It was sent to me by Professor Donaldson, of Baltimore, and it is used extensively in America for the production of local anæsthesia. It boils at 70° Fahr. Dr. Sedgwick will direct a large spray of this on my arm, and, as you see, the process of freezing is immediate. The part, a moment since flexible, warm, and sensitive, is now hard, cold, and absolutely insensible. There has been no redness, no pre-exalted sensibility, no evolution of heat—in short, no stage of *pre*-action. All the parts have been rendered inert together, and when the cold is withdrawn recovery is equally rapid, and is without *re*action. Some day we may have a means of extending this rapid inertia, so as to make the whole of a limb insensible and to operate on a part some considerable distance from the point where the application is made. This process of rapid freezing is the best and safest.

When the process of sudden freezing is brought to bear on the whole cerebro-spinal system of a cold-blooded animal, every function of life is immediately suspended. Here is a large powerful frog. I direct the rhigolene spray on the head and spine for a few seconds. I place the animal thus petrified in your hands; it is like marble—a hard frozen mass. You would declare it dead; it only sleeps. As it takes up heat from the surrounding air, its pulses recommence; in a short time it leaps; it lives again, and it sustains no injury. If you ask me how long a frog could be kept in the state of temporary death we have here seen, I answer that time seems to me to make no difference as to results, the conditions being sustained. I have every reason to believe that if the animal were kept thus frozen for a century, or any number of centuries, it would live again as well as ever on restoration of warmth. By this freezing we have fixed all the water of its tissues; we have stopped evaporation, we have suspended motion, we have arrested waste; but we have done no injury that may not be removed by the re-solution of the water and by the re-communication of motion, from the act of re-solution, to the particles of water.

We may illustrate the phenomena thus observed by experiment on a lower mechanism; and, although the illustration is necessarily extreme, it is useful, as familiarising the mind to the study of physical effects proceeding from a common cause. I take my watch and find it is in motion; its hand move under the influence of force derived from the main-spring, which main-spring is enclosed in a small box lying at this point. I bring extreme cold to bear on the box holding the main-spring; I change in this manner the condition necessary for the work of the spring, and the watch immediately ceases to go. It is not physicially injured, but it is stopped, and it would remain stopped for any length of time, the conditions being precisely the same. I change the conditions; I allow the watch to absorb the surrounding heat, and in time of itself it goes again. In the watch the cold stopped the motion by modifying the molecular condition of the metals. In the frog the water stands in the place of the metals, and the cold stopped the motion by acting upon the water.

Hybernation.—It is impossible to observe the phenomena of prolonged and profound insensibility produced on the frog by cold, with the entire recovery that follows, without recalling the simple and analogous process brought to bear on animals by Nature herself. In the process of hybernation Nature not only displays to us the same order of phenomena, but uses the same means for the production of the inertia—viz. cold—and the same means—viz. warmth—for the restoration of motion. And as, in nature, some warm-blooded animals are put to sleep by cold (the dormouse, for example), so in these we may predict that art will be able to bring about the same condition. In fact, in a bird, as we saw at last lecture, the thing was done. It will be an interesting study for some future comparative anatomist to discover why animals that hybernate come so readily under the influence of cold. There must either be some peculiarity of construction, which enables the cold to reach and act upon their nervous centres which special activity, or the nervous centres themselves must be specially susceptible to the influence of cold.

PRACTICAL INFERENCES.—The observations which these researches on the influence of extreme cold on nervous functions call forth are so varied and so rich in suggestion, it is difficult for the mind rapidly to take them in all their fulness. Vasco Nunez, standing on the height of the Darien, and exclaiming, as he saw for the first time the Pacific Ocean, "The sea! the sea!" could hardly have cause for wonder more. We are brought, as it were, to confines of reality, where before there was obscurity only and doubt and dream. We feel that in time the physiologist will pass by the poet, and, as he proceeds in discovery, will place fact in that high place which, as yet, in the history of the world, fancy has claimed as her especial domain.

Such few points of practical moment as I may on this occasion bring and string together are, of necessity, as minute as the time in which they have been studied has been short. Still they will awaken the attention, as I hope, of many, and fascinate a few.

Influence of the Conditions of the Water of Nervous Matter on Nervous Functions.—The first subject to which I would invite your attention has reference to the influence of water, under varying conditions, on nervous function. You will see by the table before us that water forms above 80 per cent. of nervous structure; albumen forms 7 per cent.; the remainder is fatty matter and salts—mainly phosphatic salts. It seems to me, from the experiments which I have conducted, that all the phenomena of disturbed natural function we have seen depend chiefly on modification in the physical condition of the water; on the transference of the water from the fluid to the solid state. In freezing nerve-matter we take from the water its heat of fluidity, or the force which, holding its molecules apart and giving them motion, supplied the condition for that mobile and active state which is the fluid state of water. We have crystallised the water of the nervous matter in all the cases in which we have frozen it. We have reduced it by this means from activity towards inertia; therewith we have deprived the structure of the power to maintain what is called life. The proof that upon the water having undergone solidification the principal phenomena rest, is proved by the fact that when the crystallised frog is warmed again, when it is allowed to take up heat from the surrounding air, it recovers its powers. In these animals the tissues are so thin, and such fair conductors of heat, that the heat can immediately act upon the nerve-substance, and can set the solid mass at liberty. In this respect frogs and animals, constituted as they are, differ from warm-blooded animals. Warm-blooded animals, evolving more heat from within and requiring more heat, are usually protected naturally with non-conducting skin and fur, or feathers, so that their heat is sustained by being retained in their bodies or conserved. But, because they are thus protected from undue loss of heat, they are also rendered incapable of directly taking up heat in sufficient degree to restore instantly the heat of fluidity of their fluids and tissues when that is withdrawn. In chilling their nervous centres, therefore, we have to be most careful so to limit our operation as not absolutely to stop the respiration and circulation. We may render them insensible in the profoundest degree; we may bring them so nearly to death that they seem dead, and if we permit the merest remains of their fire-producing apparatus to continue in play they will recover. But it is essential that we let the fire remain alight. In hybernating animals, during hybernation, this is the secret of their recovery —the fire never goes quite out.

In speaking of the crystallisation of nerve-matter by cold, I have ventured to insist firstly and chiefly on the solidification of the water; but in nerve-substance there is also a considerable per cent of fatty matter, which when heated is fluid, like water, and which also, like water, loses its heat of fluidity, crystallises, and becomes solid by cold. When, then, we freeze the brain, we solidify this fat also, and, what is more, we solidify it at a temperature at least thirty degrees higher than the freezing point of the water; and as this fat, solidifying first, becomes a bad conductor, so it impedes and limits the freezing of the whole mass of nerve substance. In hybernating animals I should think the fatty part of the brain and cord is intensely solidified by the cold.

This form of crystallisation of nerve-matter, whether of water or of fat, is simple; it is, however, not the only form of solidification that can be induced. We can solidify nerve-matter so as to destroy its function by two other means at the least—viz., by withdrawing the water from the albuminous constituent of nerve, and by altering the constitution of the albumen by heat. I can illustrate this subject by a very simple experiment. In this beaker there is a solution physically the same in composition as brain-matter; it is in a state of fluidity. I take up a little of this fluid with a soft brush, and paint a surface of glass plate with it. I now, on the opposite side of the plate, project rhigolene spray, producing intense cold. You see the fluid becomes white and solid, and adherent to the glass. It is crystallised and solidified. I take another portion of the same fluid, paint another glass plate with it, and this time, instead of using cold, I drop on a little absolute alcohol. The alcohol as it intermingles seizes the water with evolution of heat, and again there is whitening and solidification of the fluid, with adhesion to the glass. In this case, the molecular condition being changed, the albumen is precipitated, and assumes, with the water, the solid state. On a third plate of glass I put a little more of the fluid, and then gently apply heat; once, again, observe the same objective phenomena—whitening, solidification, and adhesion to the plate. Here, again, it is the albumen that has undergone change ; it, with the water diffused through it, has produced a solid mass—a condition also akin to crystallisation. If I varied this experiment by directing the same processes to living nervous matter, I should induce similar series of phenomena on the nerve-substance ; and in so far as alteration in the function of the nerve is concerned, I should produce disorder of function in every case identical. One difference—and it is an essential difference in some respects—would be presented, and it is this. If the nerve were solidified by cold, it would quickly regain its function under the influence of heat ; if the nerve were solidified by alcohol, it would slowly and imperfectly regain its function ; while if the nerve were solidified by heat it would, according to our present knowledge, be destroyed in regard to function altogether.

We cannot leave this part of our subject without being led to the contemplation of the influence of other and more refined agencies acting upon the cerebral and spinal matter. What is that change in nerve produced by a blow or communication of great mechanical force: nervous shock? What is the change produced by pressure? What is the nature of the change induced by loss of blood, by choleraic flux, by the heat of fevers, by what are called powerful odours, by strong external impressions—alarms—acting on the brain? When we see how the molecular condition of nerve can be transformed and the function of the injured part modified by cold and by heat, by abstracting and intensifying force, we gain, I think, a physical reading of these other influences as yet, of course, obscure, but still with light stealing into the obscurity.

Caloric the Form of Force in Nervous Matter.—A point which calls for special remark relates to the nature of the force with which the nervous matter is supplied. This subject has occupied the minds of the first of the thinkers of the world, and I know of no subject more absorbing or more wonderful. The experiments of Galvani and Aldini on the influence of electrical action on muscular motion through nerve led, in the early part of this century, nearly all the physiological world to the belief that in the natural nervous system electrical force is developed, and that the nerve cords from the centres are the veritable conductors of electric currents. To this day the same view has been maintained with much persistency, and various analogies have been set up between brain force, and that form of electrical force called galvanism—analogies, as I humbly think, having no real foundation in fact. In the year 1860, in the course of Lettsomian lectures "On Certain of the Phenomena of Life," I ventured to oppose some of these views, and to maintain that in the animal body there was no arrangement for the generation or liberation of any variety of force except caloric. I knew then none of the very singular experiments with cold which we have seen at the last as well as at the former lecture, but I reasoned that there was only one process in the organism for the production of animal force, and that this force, generated, or rather set free, in the combustion of blood, was an active force in every structure. Proceeding from the general to the particular, I laid down as a proposition that caloric is the primary cause of motion in nature, and is therefore a primary cause of life, in so far as motion represents life. To illustrate the position thus assumed, I was at that time compelled to resort to less direct modes of illustration than at this moment are necessary. I looked at the sun as, by the universal voice of mankind, the prime source of all living power, and I contended that the force by which he manifests himself throughout the living world is the force which in its effects, as sensible effects, we call heat, and which in all its characters, whether sensible, latent, or specific, we include under the more comprehensive term *caloric.*

I said that every observation we make in our natural lives leads to these inferences. We measure the amount of life, animal or vegetable, throughout the universe; and we learn that the proportion of life and of motion goes with the heating power of the central luminary. From the extreme pole to the burning equator, the eye glances in the imagination of absolute knowledge; and as each degree passes under review from the sterile centres of thick-ribbed ice to the intensified summers of the tropical gardens, we see the gradational life increasing until it reaches the maximum of luxuriance, and that stage of rapid growth and decay, when life and death play into each other with such force, that the one is, I had almost said, lost in the other; and viewing the scene, philosopher and simplest observer each says, "these are the effects of heat." We turn from this grand panoramic picture to our own smaller doings, and we find ourselves instinctively summoning into play a force to supplement nature—our designs are successful. We bathe our bodies in the rays of the fire when the sun retires, and we live. We wish our flowers to grow, and our fruits to run into maturity before their seasons, that our tastes may be satisfied; we make a furnace in our gardens, and our object is gained; we know here that the sensible caloric, the heat we have elicited, is the supplement of the sun. We enter into the laboratory, and we desire there to tear matter into fragments, or to conjoin materials which before were fragmentary. We dissipate solids into gases, and weld the fregmentary metal into a huge mass, an arch for a bridge, or a battery for war; and again we do it all (for we could not move a step otherwise), we do it all by the agency of caloric. We diffuse this agent through expansible water, and give to every cubic inch the dimensions of a cubic foot; the full expansion gained, we extract this expanding agent, condense the water to its original size, and by the alternation generate motion, thus imitating the great winds which, expanded in the equator, condense towards the pole, and in perpetual circuit make their way; and thus likewise imitating the gentle rain, which, distilled from the earth into the colder regions of space, condenses there and returns to its resting place in liquid drops.

We thus see in this caloric a great agent, possessing all the attributes by which the varying and yet universal properties of all matter are developed.

It is observable that in natural thought we never dream of attributing the changes noticed to any other force than caloric. It is only when we enter into philosophical subtleties that we begin to hear of other and correlative forces, as primary to caloric or substitutional for it. When, again, we turn to these profounder though probably not safer contemplations, we learn with equal sense of truth that, after all, caloric is the primary force—the first and last in action, and the universal in presence and production.

Carrying out the argument further in its especial application to living beings, I added what follows :—

"Limbs are flexible and moveable at 96 degrees of heat. If this temperature be reduced a few degrees, the muscles become painfully contracted; if the reduction be made further, the muscles become rigid; but they are relaxed by gentle warmth, and the motion is restored.

"In the animal world, I see different animals of different degrees of motive power and endurance. Examining their construction, I find as an unvarying law that the strength or the power of motion is in exact relationship to the power of the animal to eliminate and apply caloric.

"I see an animal at rest, and I notice that he converts a certain weight of carbon into carbonic acid, and liberates a certain proportion of water. I see this animal in active motion—a horse galloping with his rider, every muscle in full action; and now he pours out a greater measure of carbonic acid and volumes of visible vapour of water. Between the steam evolved by this animal and the steam evolved by a locomotive, will any one define to me the difference in relation either to cause or to effect? I mean, of course, in relation to the physical differences; for with the metaphysical I have nothing to do.

"In so far, then, as motion represents life, caloric is the source of living motion. It may undergo modifications in character; now being latent, now sensible; now being rapidly conducted through metals, or other conducting media; now rapidly evolved in series of concentrate sparks. We may call it in these varied forms by other names—*electrical force*—*galvanic force*—but it is the alpha and omega of them all—the principle of motion."

Lastly, in connexion with this universality of distribution of animal force, I inferred its unity in the animal organs, conceiving the active healthy living structures to be all stored, as it were, with force. Thus I showed "that, whenever muscular action is called into play by interference with the nervous cords, whether in the branches or in the spinal trunk itself, the excitation produces its effects by interference with the nervous column at the part irritated; not by propagation of a current through the nerve-trunk leading from the excited part to the deranged muscle. For instance, when I passed a continuous current through a portion of the spinal cord of an animal, I for the time produced a paralysis of the muscles which immediately communicate with that section of the cord; but that this is not an effect produced by a transmitted current to the muscles was proved by the fact that, by galvanizing the nerves between the cord and the muscles, I could cause contraction of those muscles.

According to the view I then held, I also inferred from all the phenomena observed that the nervous system is in every part a producer of the peculiar force with which it is endowed; not that the brain or ganglia are special producers; not that a current from these centres, intermittent or continuous, is traversing the nerve-fibre; but that the nerve-structure, so long as it is supplied with blood, is producing the force wherever there is nervous filament. I looked on the vast area of nerve-fibre in the peripheral surface, and I saw in it a mass equal to that of the brain; I saw this mass supplied with blood everywhere, and built always on the same plan. I assigned to it everywhere the same purpose and labour.

In this way I was led to look on the muscular system as an entire independency, and on the nervous system also as an entire independency. The muscular system, nourished by blood and charged with caloric as caloric; the nervous system, nourished everywhere by blood, and changed also with caloric in its electrical modification;—each independent systems. We conjoin the systems, and the result of their equilibrium is a simple passive state, while the result of a disturbance of their equilibrium is motion and sensation.

"Thus," I reasoned, "as every portion of nerve down to the minutest branch possesses producing power, the mass of the force generated is so universally distributed, that interference in any part of the nervous communications is reflected to the whole nervous system. So when our distinguished brother, Dr. Brown Séquard, produces artificial epilepsy, and induces the paroxysm by irritation of some particular external point of nerve, he does, in fact, in that irritation touch at one presenting point the universal fluid pervading the whole body of his subject, and excites, not by special transmission, but by general disturbance of the equilibrium of the forces, a convulsion through the whole muscular organism. So, when with the intermittent current I galvanise a portion of the nervous tract, I produce convulsion, because I induce an alternation of force; at one moment allowing the natural equilibrium to establish itself; at the next moment disturbing it. So, when I continue the current without intermission, I virtually cut off altogether the included nervous tract from its system and cause paralysis of will, because I have cut off also communication with the brain; but I can, nevertheless, call into play at pleasure the excitability of the nerve trunks below, as long as they continue to summon into their service blood for their nourishment and force-producing faculty.

"If it were possible to entirely remove from the body every muscular fibre, and leaving the nervous system entire, still to supply that system with blood and surround it with those conditions under which its blood could be applied, that nervous system would exist as a motionless intelligence. It might think, feel, and by virtue of its sensual organs appreciate and know the external world surrounding it; yet be incapable alike of act or of expression. On the other hand, if every particle of nerve-matter could be removed, the muscular system being left with its attachments to bone still secure, and its blood-current free; that muscular system would remain an unintelligential mechanism, having in itself its *vis insita*, but being incapable of exerting movement until brought into action and guided by the intelligential part of a more perfect animal.

"By the combination of the two systems in the perfect organism we obtain, so long as the necessary conditions for life are supplied, the doubly endowed and self-acting body. An excitation of light refracted on the nervous expanse of the retina touches the pervading force, and the animal sees; but this light must be presented to the nerve-expanse, or, in other words, to the force that pervades the expanse, in such a way that the absolute physical picture shall be put upon it, or the picture will not be seen. It is not that the picture is to be carried to the brain, but that it is to be looked on at this point of the nervous expanse by the presiding force. A vibration is set up in a mere physical membrane, spread above another distribution of nerves, and the animal hears; it is not that anything is conveyed specially to the brain, but that the equilibrium of the pervading force is disturbed. An impression is made on the skin, and the animal feels; it is not that any current is conveyed to the brain, but that the impression disturbs the balance of the nerve-fluid throughout its universality. The impression made is slight, and it is pleasant, or not painful; it is severe, and it excites the whole animal body, so that the body writhes in agony, and may even die from the reflection of the impression upon the muscular fibre, and the resultant spasm."

The views expressed above, at the time when they were first enunciated, were based confessedly on general inferences. They were not sufficiently exact to satisfy the demands of strict science, and for that reason they were never unduly pressed on the attention of the profession. At the same time they were, in the main, correct, and now, somewhat simplified, they stand firmly, I believe, on fair and rigid experiment.

For what are the facts which have here experimentally been brought to our notice? We have had before us a frog; we deprived it of all sensibility; we deprived it of all power of motion. How did we effect this? Simply and solely by the abstraction of the heat from its cerebro-spinal system. When the animal was thus reduced to inertia—when to all appearances it was dead—we saw its life fully restored. In what manner, under what condition, did this restoration become pronounced? There was only one condition—restoration of caloric. When we put these two great facts together, and when we couple them with the further fact that the animal has no means for producing any force except calorific force, we cannot, I think, escape the conclusion that animal force is caloric. With this we may admit—indeed, we must admit—that other correlatives, when brought to bear on the living body, can stir into temporary motion the organic structures; we must admit that light can set up motion from the eye to the brain; that the passage of an electric current can set up motion in muscle, and motion from any periphery of nerve to brain, that the vibration of air can convey motion by the ear to the sensorium, and that the motion of minute particles of organic matter may be communicated through the olfactory nerves back to the centres of those nerves. But these admissions do not interfere with the general truth that caloric is *the* force of life—that it is true and sufficient to explain all the phenomena of living motion.

Before this curious experiment of producing inertia in a living animal by abstracting the caloric from its nervous centres, is allowed to pass from our thoughts, let me make one more reflection respecting it. The inert animal is either asleep, or it is dead. The terms sleep and death are, I know, relative, but as we must be guided by them, and as we know the animal can be restored, we are bound to say of it that it is not dead, but that it is as we are, when we are shut out, for a time, from the world—asleep. We let back then the force that has been taken away; as the force goes back, it loosens the hardened tissues, it enters the structures; it enters the nervous centres, it recharges them, and thereupon there is renewed motion. We learn, thus, that when these nerve-centres are reduced in force there is a powerless organism; we learn that so reduced the centres can again take up and again lay up force; we gather the truth that when the centres are once more replete with force they communicate the fact to the rest of the organs, and that the body therewith awakes. Of what is this the counterpart? I think it to be the counterpart of natural sleep. I take it that during sleep the exhausted brain, the exhausted nerve-cord, the exhausted nervous system, everywhere takes up and stores up caloric, and so continues to take up and store up until it is charged to its full capacity. Then, if I may use such expression, it overflows with force, it spontaneously fills the body, and there is presented that phenomenon of motion which we call "the awakening." Send other force, vibration from noise or mechanical motion, at any moment through a sleeping body, and you may, through a dynamic act, excite motion, and the body may awake; but by this you have not primed the body with the force it wants for sustained work; you passed a charge of force through it, but you did not charge it. So when my unwound watch is ceasing, I can stimulate it into movement for a moment or so by a moderate blow or shake; but the force is applied uselessly if the mainspring be not recharged.

The phenomenon of sleep and the phenomenon of awaking from sleep appear thus to me to have a simple and a rational explanation—an explanation purely physical in character, and which may be applied to the correct exposition of many of the phenomena of disease as well as of health.—*Medical Press and Circular, June* 19, 1867, *p.* 575, *and Med. Times and Gazette, July* 20 *and Aug.* 3, 1867, *pp.* 57, 113.

13.—ON A NEW METHOD OF TREATING PARALYSIS BY THE APPLICATION OF COLD OR HEAT ALONG THE SPINE.

By Dr. John Chapman, Physician to the Farringdon General Dispensary.

The term paralysis denotes in reality various diseases, diseases which differ alike from each other in respect to their phenomena, the parts of the nervous system in which they are seated, and the nature of the morbid condition which obtains. In fact, they differ from each other so decidedly that their only common element consists in disorder of some part of the nervous system, disorder most frequently, however, extending to the voluntary or involuntary muscles, or to both, at the same time. The morbid states of the nervous system are also due to widely different, and not seldom indeed to quite opposite, proximate causes—as, for example, anæmia, hyperæmia, congestion, inflammation, red softening, white softening, hemorrhage into the nervous centres, pressure of tumours, tubercle, cancer, and finally, blood-poisoning—as in cases of syphilis, uræmia, plumbism, and the eating of bread containing the ergot of rye. There are also the cases of paralysis resulting from mechanical injuries, and finally the progressive paralysis of the insane. In fact, it seems to me that the nomenclature of so-called paralytic diseases is extremely defective; for at present it is customary to designate morbid states of the muscular system which are exactly opposed to each other in nature by one and the same word. The flaccidity and atrophy characteristic of the muscles in cases of wasting palsy, present a complete contrast to the rigidity and comparatively undiminished bulk of the muscles in other forms of motor paralysis scarcely less notable.

[The writer proceeds to relate a case of paralysis involving the muscles of the tongue, throat, back, and four extremities, and associated with giddiness, headache, profuse dribbling of saliva, constipation, and scantiness of the menses. The patient was a married woman, 42 years of age, and described as "a thin, nervous, and intelligent person."]

When first prescribing for her, I ordered aperient pills to be taken occasionally at bed-time, quinine and sulphuric-acid twice a-day, and requested that the shoulders, arms, back, and legs should be rubbed vigorously as often as practicable, and that towels dipped in iced-water, and so folded lengthwise as to be about four inches broad, should be applied along the whole spine during twenty minutes, three times a-day—the towels being carefully changed immediately they became warm. This report would become inconveniently long were I to give a detailed account of the treatment I adopted during the time the patient was under my care, I shall therefore restrict my description of it to a general statement of it as follows :—Besides the medicines just named, the only others used were calumba, citrate of iron and quinine, nitric acid, an aperient draught, and bicarbonate of potash. Galvanism was applied to the muscles—chiefly of the extremities, and was used for the first time, May 15, 1863. I also applied heat freely, by means of hot water, to the extremities; but the main treatment, and that upon which I principally depended, consisted in lowering the temperature of the spinal region, by means sometimes of iced-water, sometimes of the spinal ice-bag. The applications were, at one time, along the whole spine, at another to a part, and for different lengths of time, the variations being made according to indications observable from time to time.

Results of Treatment.—The patient showed indisputable signs of improvement within the first week of treatment, which was continued upwards of two months, during which she progressed most encouragingly. Up to the end of that time the change effected in her may be summed up as follows :—She could hold her head erect; could protrude the tip of the tongue beyond the teeth full half an inch; the dribbling of saliva quite ceased; she could speak much more distinctly; could swallow fluids without difficulty, and drink a cupful of tea or coffee at a draught without becoming choked, or spilling any of it; she could raise the right arm vertically—placing the elbow higher than the head, and the left arm so that the tips of the fingers reached as high as the top of the head; the arms increased in size,—during the first eighteen days of May the diameter of the middle of each forearm increased a quarter of an inch; and the power of each arm was correspondingly increased. Even as early as the 18th of April she said :—"My arms are wonderfully much stronger. I can feed myself; can pull myself up, and can wipe my face," &c., and with joyous pride showed me that she could lift a chair—a feat she could not perform the previous week. The claw-like set of the fingers disappeared. On the afternoon of Sunday, May 3rd, the patient experienced a sudden increase of power in the extensor muscles of the left forearm. Whereas, for the first time, I could distinctly feel their contractions beneath my fingers; and the pulling and grasping power became, during treatment, markedly greater than before. The concavities formed by the wasting of the ball of each thumb were in no perceptible degree filled up; there was, however, a slight though decided increase of motility of each thumb, and the left flexor longus pollicis became markedly stronger, notwithstanding that the concavities just named continued. The metacarpal interosseous spaces of each hand were gradually filled up to a surprising extent, so much so, indeed, that her hands became quite plump. The power of flexing the thighs on the trunk and of separating them from each other gradually returned, so that instead of slipping down from her chair continually, as she had formerly done, she was able to maintain her position, and, finally, to move her thighs about freely in all directions. The feet gradually became susceptible of passive motion, and afterwards she was capable of putting both feet flat to the ground, and of moving them in different directions—outwards as well as inwards—by the mere force of her will. During the latter end of May she stood occasionally for two or three hours at a time, supporting herself sometimes only with one hand, and one day, with the help of her son, she walked into an adjoining room. The muscles of the back, with the exception of a few of the upper fibres of the trapezius, ceased to quiver, and re-acquired a very considerable development. In fact, she gained flesh generally to such an extent as to cause her nurse to complain, during the latter portion of her attendance, of the great additional weight of the patient when moving her about. This increase of flesh was even more remarkable in the face than in the rest of the body, so that, compared with what she had been, she appeared quite plump. The back became permanently much cooler than it had been; the function of the bowels healthier; the catamenia abundant; appetite greatly improved; the head clear, and not only free from headache, but free from the great giddiness which, up to the time when my treatment began, she had experienced during the whole period of her illness.

At the end of May pecuniary and domestic troubles which had long pressed heavily upon her reached a crisis, which exerted such a depressing influence upon her as not only to stop further progress, but actually to throw her backwards. During the long period of her illness her husband, who is a working man, had been obliged to pay some one to do the house-work and to attend on her, and having become involved in debt he now sent away the person who had done this work, and broke up his house. His wife was then taken to her relations, at her native place in the country, and I never saw her again.

It would be difficult, indeed almost impossible for me to select a case more suitable than this to exemplify the efficacy of the therapeutical method, which consists in modifying the temperature of the spinal region; for this patient presented a combination of symptoms, each of which may be fairly regarded as a grave disease, and each of which was influenced beneficially and to a remarkable extent; there was paralysis with flaccidity and wasting of the affected muscles; there was also paralysis with rigidity of the affected muscles; there was headache, with giddiness, especially when the patient stood up; there was profuse dribbling of saliva; there was loss of appetite; habitual and extreme constipation; and the menses were very scanty. Now it appears to me that a skilful interpretation of these various symptoms would lead to the inference, that the spinal and collateral sympathetic nervous centres were in that morbid condition, which indicated that the application of cold would exert a remedial influence. The history of the case seems to show that the disease originated by injury to the hand, that the morbid condition induced by the accident result in a transmission of a morbid influence to the spinal cord; and that this influence was diffused both along the cord, and over the sympathetic nervous centres. As might be expected, the disease was most intense in that region of the cord where it was first generated, so intense, indeed, that probably portions of the cellular constituents of that part ultimately became more or less atrophied or destroyed. Simultaneously the collateral sympathetic ganglia were kept in a state of morbid excitement by the influence of the disease in the cord, and thus exerted an excessively constringing force on the arteries under their control. In this manner, I account for the particular form of paralysis with which the muscles of the throat, neck, upper extremities, and dorsal region were affected. While, on the one hand, that healthy influence dominating the nutritive processes which is diffused from the cerebro-spinal system throughout the organism was, in respect to the parts in question, greatly diminished and probably also perverted, and while the power of transmitting motor impulses to the muscles implicated was impaired, the supply of blood to these muscles was so greatly lessened, owing to the chronic spasm of the arteries distributed to them, that it was no longer possible for them to receive adequate nourishment. The conditions here described are precisely those which are calculated to insure muscular atrophy; and, I venture to suggest, that that peculiar feature which frequently distinguishes wasting palsy, viz.,—the wasting of special groups of muscles, and groups, moreover, which are often related to each other by co-ordinate action, originates in the fact that special series of nerve cells occupying limited areas within the spinal cord, and functionally related to the group or groups of muscles implicated, are diseased, the rest of the cord remaining healthy, or at least disordered only to the extent of hyperæmia. In this case the result of the diffusion of the morbid influence down the cord and to the collateral sympathetic centres was, as it seems to me, the production in those parts of hyperæmia, so considerable as, by greatly intensifying their functional energy, to induce a tetanoid condition of the lower extremities : the energy of the spinal cord transmitted to them was continuously excessive, thus inducing in them a state of continuous contraction. Simultaneously the stimulus diffused from the sympathetic ganglia over the arteries of the lower extremities, so far exceeded the normal amount, as to lessen in some degree the supply of blood to the muscles, and thus insured the condition which, as is well known, is peculiarly favourable to the onset and continuance of spasm or muscular rigidity. On the other hand, while the nervous centres related to the arteries in question were so far unduly active as to produce the condition just described, they were not acting with that intense degree of morbid energy which would result in almost wholly cutting off the blood supply, and which, as stated, was the case with respect to the nervous centres related to the upper part of the body. Hence, while the muscles of that part were extremely wasted, some of them being almost wholly destroyed, the muscles of the lower extremities were not diminished to any considerable extent.

If it be admitted, as indeed it must be by all who have experience in this matter, that the cerebral blood-vessels are under the government of nerves emerging from the sympathetic within the region limited above by the fifth or sixth cervical vertebra, and below by the third or fourth dorsal, and if it be further admitted that in the case in question the sympathetic centres within this region were morbidly affected, we should be led to predict that the cerebral circulation would become disordered, and that the disorder itself would present the form of cerebral anæmia. This, in fact, was the case : there was headache, associated with feeble mental action, and frequent giddiness, especially when the patient stood up.

Seeing that there were symptoms of grave disease of the spinal cord in that region where the nerves to the upper extremities are given off, it is easily intelligible how the uppermost part of the cord, together with the medulla oblongata, would, through the propagation of the morbid influence upwards, become hyperæmic. Now the nerves which cause the parotid and submaxillary glands to pour out their appropriate secretion, are motor-nerves from the cerebro-spinal system, viz., branches of the auriculo-temporal and the chorda-tympani, and the origin of these nerves is the medulla oblongata. These facts being recognised, it will be at once seen that hyperæmia of that nervous centre is likely to result in an excessive flow of saliva; and such in fact was the case with the patient in question.

Extensive experience has taught me that an abnormally irritable state of the spinal cord and collateral sympathetic ganglia along the lower half of the dorsal region, is peculiarly conducive to functional disorders of the stomach—loss of appetite, indigestion, nausea, vomiting. And I have long been of opinion, founded on many observations, that one at least of the immediate causes of nausea is a great superabundance of mucus in the stomach, owing to the excessive stimulus transmitted from the spinal cord to the mucous glands of that organ. Assuming my diagnosis of the case under consideration to be correct, it follows that there was hyperæmia of the whole of the spinal cord, below as well as above the part first diseased, and therefore an intelligible cause of the symptom—loss of appetite; and, as I shall now proceed to show, of the extreme and habitual constipation from which the patient suffered.

If my ideas concerning the pathology of constipation be correct, it may proceed from three causes—1st, from partial atrophy of the circular muscles of the intestines; 2nd, from inadequate nervous stimulus of those muscles; 3rd, from a deficiency of intestinal mucus. Now, as I contend (and I must content myself for the present with merely stating my conviction), both the muscles and the mucous glands derive their functional energy from the spinal cord; if so, the second and third causes just named of constipation, consist in enfeeblement of that part of the cord related to the intestines. This condition is far less frequent than that of excessive spinal irritability, the result of spinal hyperæmia. It follows, therefore, that constipation due to spinal anæmia, and therefore enfeeblement, is comparatively rare. On the other hand, cases very frequently come before me, in which obstinate and habitual constipation is associated with indubitable symptoms of hyperæmia of the nervous centres along the lower dorsal region. In these cases the production of constipation is, I apprehend, as follows:—The nervous energy diffused over the mesenteric arteries being excessive and continuous, forces them into a state of chronic contraction, many of the smallest ones being often, I doubt not, closed entirely. The consequence is, that the intestinal muscles cease to be adequately nourished, and therefore, losing their wonted strength, become incapable of effecting those vigorous vermicular contractions on which the power of the alimentary tube to propel its contents with healthy force and regularity depends. It is conceivable, and indeed probable, than when, owing to chronic contractions of the mesenteric arteries, arterial blood is partially shut off from the intestinal walls, the mucous glands will receive less than their due supply, and that therefore, while those glands are being adequately stimulated to fulfil their functions, they are incapable, from want of blood, of elaborating a sufficiency of mucus; if so, two distinct causes may co-operate in producing the constipation in question. If these views be sound, we should be justified in predicting that, in the case just described, the character of the paralysis would be associated, as it was, with persistent constipation.

The last symptom to which I have to advert was scanty menstruation. Reasons have already been given for believing that the nervous centres in the lumbar region, as well as those above, were hyperæmic; and I have demonstrated by a large amount of evidence that, if the nervous centres along the lower half of the spine are rendered hyperæmic by the application of heat, the menstrual function may be retarded, lessened, arrested, and in some cases, at all events, completely suppressed. It will therefore be readily understood how the special menstrual disorder of this patient was one of the results of that general disease of the nervous system from which she suffered.

In seeking to understand the nature of things, it behoves us, as has been well said by Newton, to admit only such causes as are at once true and adequate to explain the phenomena in question. In what I have said concerning the foregoing case, have alleged that one morbid state of varying degrees of intensity in various parts of the spinal cord and sympathetic was adequate to produce all the symptoms passed in review, and have attempted to explain, *seriatam*, the *modus operandi* of their production.

Of course, if my explanation of the proximate cause of the symptoms be correct, it follows that the most scientific method of removing that cause would consist in exercising a sedative influence on the nervous centres morbidly affected, and on no others. My treatment of the patient was the result of this reasoning, and considering how signally several other attempts to relieve her had failed, and the desperate state in which I found her, the benefit conferred is a striking testimony to the efficacy of the plan adopted. The medicines already mentioned, which were administered, played, I apprehend, a very subordinate part in the remedial process; the friction which I ordered to be applied to the wasted muscles with a view of facilitating an increase of their nutrition, as well as the galvanism which was applied chiefly to the lower extremities, probably co-operated more effectually; but my experience in many other cases enables me to state confidently, that the great improvement which was effected in this patient was mainly due to the application of cold along the spine.

[The next case was one of paralysis (hemiplegia and paraplegia) associated with epilepsy and inducing great impairment of speech, extreme incontinence of urine, excessive drowsiness during the day-time, and extraordinary coldness of the surface of the body, especially of the extremities, even in summer. The patient was a man 56 years of age. When he was 30 years old he had a severe epileptic fit, but there was no recurrence until he was 40 years old, when he complained of a feeling (pressure) in his head and immediately fell; he was much convulsed, and bit his tongue. Soon after this, without losing consciousness, he became nearly paralysed on the right side of his body, the speech being also affected. This was in great measure recovered from but not completely, and again became worse when he had another attack in his 53rd year, at which period he came under Dr. Chapman's care.]

I ordered ice to be applied along the whole spine during two hours, five times during each twenty-four hours, and requested that the spinal ice-bag should be enclosed in flannel; I also prescribed five grains of bromide of ammonium twice a-day.

June 18. Has had no fit, and does not feel likely to have one His head feels better, feels lighter; "he don't feel," his wife says, "that heaviness that he did." Sleeps well at night and less in the day-time than before. Has not complained at all this week of the numbness in his hands: his wife says they are decidedly better. The weakness of the legs continues, and is, she thinks, greater than last week. The bowels have already become "very regular, once a day, and sometimes twice," since he began to use the ice. He retains his urine longer than formerly. Feels warm all over—the feet being especially warm.

25th. No fit; he says the "head feels very well; feels clear and cool; I don't seem to have any fear of a fit;" the speech is clearer and more distinct; the sensibility on each side of the face is now equal; he feels two points at three lines apart round the angle of the mouth on each side; the right arm has decidedly gained in strength; and the sensibility of both hands has become normal; he now feels two points at four lines apart in each palm; the lower extremities are also stronger; he can now turn the left toes outwards; bowels regular; he notices a a great diminution in the frequency of making water; continues constantly warm all over; to apply the ice as last ordered four times a day instead of five, and to continue the bromide of ammonium.

July 2nd. No fit; sleeps well at night, but *much less* in the day time than he used to do; bowels regular. He now makes water about four times during the night, and six during the day. To apply ice as before three times a day instead of four, and to enclose the spinal ice-bag in a thick worsted stocking instead of in flannel as before. ℞. Quinæ disulphatis, gr i.; acidi sulphurici diluti, ♏vii. bis die.

9th. No fit; no headache; feels himself "as well as possible, but very hot—very hot;" his speech is greatly improved; his mind also; the mental improvement is clearly expressed in his face, which is lighter and more intelligent; sleeps "capital" at night, still less during the day; feels no numbness whatever of the hands and no sensations as of "pins and needles;" can still evert the left foot, and can now stand on each leg, but especially on the left one; he can also now walk alone; bowels regular; power of containing the urine continues to increase; now passes it about four times in the day and four times in the night. To apply ice twice daily, in three folds of worsted, and to continue the medicine as before.

16th. No fit. Reports himself still better. His wife says "he does not sleep in the day like he used to do, and sleeps very well of a night." He talks still more clearly. Retains his water still better, getting up only twice, or at most three times during the night. He can now sometimes walk very well, even without the aid of a stick. To continue as last ordered.

23rd. Has had no fit, but has just fallen, having been pulled down by his wife in her attempt to get him out of the way of a carriage, after getting out of an omnibus. He is shaken by the fall, and complained for a short time of headache afterwards. Otherwise is in all respects the same as last week. Treatment same as before.

30th. Still no fit, notwithstanding the fall and fright last week. Yesterday, for a short time, the right hand became "rather shaky" and numb: the numbness lasted about half an hour; the shakiness a good part of the afternoon. He was quite well again in the evening. His head remained quite unaffected. The improvement of the bowels and bladder continues. To apply ice during two hours three times a day; the bag being enclosed in one fold of flannel. ℞. Potassi iodidi gr. ii½; ammonii bromidi gr. v; ex aquæ ter die.

August 6th. Continues without a fit or other bad symptom. To persist in the treatment last ordered.

September 9th. Had a fit on the 30th ult. at 5·30 a.m. His wife says she believes "it's worry that brought it on." She states that he had ice each day until the fit, but none afterwards. *Since he left off the ice he has become cold again.*

A careful consideration of the symptoms of this case, apart even from its history, can scarcely fail to lead to the conclusion that the paralysis of the extremities, the dull heavy headache, the defective speech, the disordered sensibility evinced by the cheeks and hands, the grave functional disease of the bowels and bladder, the excessive somnolency, and remarkable coldness of the surface of the body, are all referrible to excessive hyperæmia, merging into congestion of both the spinal cord and the sympathetic nervous system. According to my reading of the case, there was considerable congestion of the spinal cord, resulting in the paralysis of the lower extremities, and cooperating to produce the disordered sensibility especially evinced in the face and hands, as well as the extreme functional derangements of the bowels and bladder. On the other hand, the vascular condition of the sympathetic ganglia was rather one of extreme hyperæmia than of congestion, hence their preternaturally vigorous action which resulted in such vehement arterial contractions throughout the system as to deprive the brain, as well as the periphery of the body generally, of its due supply of blood. The consequence was, the mental heaviness and extraordinary somnolency and the excessive coldness which the patient experienced. Moreover, other of the symptoms were not less certainly, though less obviously, due to the same cause; the anæsthesia or numbness, and the sensations as of being pricked by "pins and needles," though partly referrible to congestion of the medulla oblongata and spinal cord, were also, I have reason to believe, in a great measure due to the excessive action of the sympathetic, which, lessening the supply of blood to the parts affected, prevented that complete nutrition and oxidation of the nervous constituents of the tissues of those parts which are essential conditions of normal feeling. The same cause resulted in the starvation of the muscles of the bowels, and, probably, of those of the bladder. Hence, according to the explanation already given, the peristaltic energy of the bowels was impaired; and hence, probably, the muscular coat of the bladder, being also weakened, became abnormally irritable in accordance with the well-established fact that badly-nourished, weakly muscles are most excitable.

Assuming this pathological view of the case, it is obvious that no treatment of it was calculated to prove so beneficial as that which was capable of abolishing at once the hyperæmia and congestion of the spinal cord and sympathetic; and as the case was one of many years standing, and therefore presenting, probably, extreme dilatation of the blood-vessels of the nervous centres in question, I ordered the prolonged use of the spinal ice-bag each day in the first instance, in order the more effectually to remedy the morbid condition in question, and, at the same time, to prevent the possibility of reaction.

It is extremely to be regretted that this patient was not enabled to continue providing himself with ice for a much longer period than that during which my treatment of him lasted; for had the blood-vessels of the nervous centres acted upon been kept vigorously contracted to their normal diameters during an adequately long period—say probably six months, I have reason to believe the patient would have derived greater, and more lasting benefit than was actually conferred upon him. It will be admitted, however, that the case as it stands, is an extremely interesting and instructive one, inasmuch as it affords the most decisive proofs of the truth of those pathological and therapeutical doctrines, which I have propounded. Within the space of five weeks, the right arm of the patient became much stronger; he was enabled to walk alone, even without the aid of a stick; to evert the left foot, and to speak very much more distinctly; his intelligence and mental expression were greatly improved; the numbness and other disorders of sensations, as well as his headache and drowsiness in the day time vanished; he became not only warm all over but "very hot;" his bowels were opened daily; his power of retaining his water so increased that he was obliged to get up only twice, or at most thrice, during the night; and, finally, whereas before treatment he had an epileptic fit about every month, one did not occur until nearly three months after the treatment began.

The fact that the patient became cold again after leaving off the spinal ice-bag is both interesting and instructive: the chronically contracted arteries were dilated to their normal volumes by the treatment adopted, but not being under its influence sufficiently long to enable the newly reacquired increase of their diameters to become established as a physiological habit, they contracted again when the spinal ice-bag was no longer applied, and hence the patient again became cold.—*Medical Press and Circular, May* 1, 1867, *p.* 409.

14.—ON RHEUMATIC PARALYSIS.

By Harry Lobb, Esq.

Perhaps of all the diseases that come under the treatment of the medical electrician, rheumatic paralysis is the most amenable to the judicious application of electricity. As a rule, ordinary routine medical treatment has no effect upon this malady, and the disease progresses from bad to worse, until the use of the limb or limbs is completely lost. The following case is a most instructive one, and is a fair type of this disease.

Mr. G. called upon me on the 20th of last March, with a note from Sir Wm. Fergusson, stating that "the bearer will tell you his story; my idea is that electricity may be of service, and I recommend him to you with that view," and giving the following history, in writing, which I give in his own words. "On Saturday, the 2nd of February, 1867, on returning home in the evening, I felt what appeared to me to be a light blow on my right shoulder, which ached slightly from that moment until I got into bed, the pain then increased in the shoulder, neck, and back of the head, and kept me awake nearly all night. From that time, for three weeks, I did not sleep more than two or three hours in the twenty-four. I took medicine to operate on the liver, and also effervescing draughts to act on the skin. Vapour baths, hot-air baths at the hammam; shampooing, rubbing with stimulating oils, and douches were tried, from all of which I experienced temporary relief, but not of a lasting nature. At the end of six or seven weeks nothing but the rudimentary rheumatic pain was left.

"About the 5th of February, I began to feel great weakness in my right arm and shoulder, and difficulty in shaving and dressing. On the 10th I employed a barber, who shaved me every morning. After the 10th of March I virtually lost the use of my deltoid and biceps, but could always write when my arm was lifted and placed on the table. I saw ——, who gave me a liniment of chloroform and camphor, which proved useless. Afterwards he gave me iodide of potassium, with the like result. My arm not improving in the slightest degree, I consulted Sir W. Fergusson on the 20th of March, who sent me the same day to Mr. Lobb."

On examination, I found the deltoid completely paralyzed, the biceps almost so; both much wasted and very soft; the extensor very weak.

Electrical Examination.—The battery I employed is my own Portable Book Electro-Magnetic Battery, made by Ladd. First using the secondary wire, or induced current of electricity, attaching a moist conductor to the positive pole, which I placed upon the spine, I examined the skin covering the affected muscles with a wire-brush attached to the negative pole, using the whole power of the battery.

Sensation was very dull over the deltoid and biceps, and there was no reflex contraction of muscular fibre under the electrical stimulus.

The conductors were now shifted to the primary wire, and two moist conductors were employed. The one from the negative pole was placed beneath the spinous process of the scapula, and the conductor from the positive pole was placed upon the muscle to be stimulated.

On passing the current through the deltoid, faint contractions were discovered in the anterior and posterior fibres, but none whatever in the middle fibres; the biceps contracted feebly; all the other muscles were in tolerable order.

Treatment.—A Pulvermacher band was applied, direct current positive pole on the spine, then wound round the deltoid, and the negative pole placed on the lower part of the biceps, and retained in position by an elastic bandage, to be worn night and day; excited once a-day with one part vinegar, and eight parts water. Daily, for half-an-hour, each muscular fibre of the paralyzed muscles to be excited with the inverse current of the primary wire of the electro-magnetic battery.

This treatment, without change, was carried on daily for three weeks, with immediate benefit from the first application, and on the 18th of April I took Mr. G. to Sir William Fergusson completely cured, enabled to use the deltoid and biceps perfectly.

This is a model case. I have had numerous somewhat similar ones; but it must be borne in mind that the longer the paralysis has lasted, and the more the muscle has wasted, the longer the treatment will be required before a cure can be effected.

Paralysis of the leg, following sciatica, is not uncommon; the treatment is precisely similar, and the result equally satisfactory. —*Medical Press and Circular, June* 12, 1867, *p.* 457.

15.—ON THE TREATMENT OF PARALYSIS FOLLOWING SCIATICA BY MEANS OF ELECTRICITY.

By Harry Lobb, Esq.

[The paralysis following sciatica is very similar to that resulting from rheumatism, and as regards the treatment is precisely the same. The following case is interesting from the rapidity of cure.]

Mr. M., the well-known veterinary surgeon, consulted me last year for a very severe sciatica of the left leg, from which he had been suffering for three months, and for which he could get no relief, the pain of a very acute kind, darting down the sciatic nerve to the foot; he complained that he had not had two hours consecutive sleep for three months, and that he was nearly worn out with pain and fatigue. He certainly appeared much worn, and had evidently suffered much pain. He was very lame, and the leg was no use to him, as he could not bend it, or bear his weight upon it. The right leg was unaffected, and he managed to get on with this leg and a stick. He had been treated medicinally with alteratives, antacids, &c., and his diet had been properly attended to. He had also tried all the known nostrums, which friends so readily press upon the afflicted as infallible.

Considering the case to be one of pure neuralgia, requiring only the continuous voltaic current to be passed through the nerve in large quantities, to bring it back to its normal state, I at once applied a current from a Pulvermacher's 120 element battery, by applying the positive pole to the lower portion of the spine, by means of a wet conductor, and placing the foot in a basin of water brought it into the circuit with the negative pole. This current was allowed to pass for about twenty minutes, when I immediately put on two No. 0 bands, the positive pole on the lower portion of the spine, and taking a turn round the leg, the negative pole was placed beneath the sole of the foot—the bands being excited with one part vinegar to eight parts water, and retained in position by means of an elastic bandage.

Mr. M. called on me the next day, stating that he had had no pain since the application, and that he had slept the whole night. I, however, again applied the current from the battery and the bands, and as a precautionary measure, continued this for a fortnight, but he had no return of pain, and after a few applications of the interrupted electro-magnetic current, to stimulate to contraction the partially paralysed muscles, he rapidly regained the use of the limb, and in about a fortnight was perfectly well, and had quite lost his lameness.

Occasionally one sees a case of paralysis of the leg following sciatica of long standing, where the pain had ceased, perhaps, for months, or even years, and where the patient has not recovered the use of the leg. There is generally much wasting of the unused muscles, and some time is required to effect a perfect cure.

The following case came under my notice in my hospital practice, and is worthy of record:—

William Pratt, 5, Buckingham-street, Pimlico—paralysis of the right leg of two years' standing, following sciatica—has still some pain in the leg; the coldness of the limb was remarkable; it had also wasted considerably, and it was stiff and useless.

Treatment.—A Pulvermacher voltaic band to be worn from the back to the foot, wound round the leg, and retained in position by a bandage, the band to be excited every morning with one part vinegar to eight of water, and to be worn day and night. A powerful current from a Pulvermacher battery to be applied, by means of a metallic surface, to the limb, direct current. This at once produced a return of circulation and warmth. By continuing the treatment for a fortnight he was quite cured, and could walk to and from the hospital with comfort and ease.

This is a remarkable case, from the length of time the patient had lost the use of the limb—namely, two years, and from the extent of the paralysis, from the wasting and peculiar coldness of the surface, as also from the rapidity of the cure. It appeared that on inducing circulation in the skin, a reflex effect had been produced upon the muscles through the cord, for a return of power was almost immediate upon a return of warmth to the surface.

I could relate fifty such cases as the above having a like result, but these two are sufficient to point out the nature of the treatment.—*Medical Press and Circular, July* 17, 1867, *p.* 54.

16.—CAPSICUM IN DELIRIUM TREMENS.

Since our last notice of the employment of this simple and efficacious plan of treatment, some well-marked cases have occurred in Dr. Lyons's practice. In one instance the patient, a tavern-waiter, of chronically intemperate habits, was admitted to the Whitworth Hospital in the first stage of this morbid condition. The patient exhibited tremor in almost all the muscles of the body, chilliness, debility, sleeplessness, foul tongue, severe and general uneasiness, but there was a total absence of illusions, horrors, or delirium to any degree. He got a single dose of capsicum, twenty grains in a bolus, after which he slept and fully convalesced, the disease having been thus peremptorily cut short. Dr. Lyons remarks on the great importance of this early phase of the disease being recognized and promptly treated. The patient is in that condition in which he may be by but slight further indiscretion plunged suddenly into all the horrors and moral degradation of the state of fully developed delirium tremens, with all its consequent loss of character with others, and loss to the patient himself of that last barrier against utter abandonment, the sense of shame and remorse. For not alone does the first occurrence of delirium tremens brand the sufferer with the character of an all but irretrievable dipsomaniac, but the fact that he has passed this moral rubicon, in the vast majority of cases deprives the patient of all stimulus to self-control, and under the demoralizing feeling that there is nothing further to be risked, his steps henceforward ever tend downwards and from bad lead on to worse.

As Dr. Lyons observes, a brief but variable period often precedes the fully developed attack of delirium tremens, especially in first cases, in which the patient presents anomalous symptoms unintelligible to himself, and not always read aright by his attendant. This stage is in some patients marked by the occurrence of tremor, sleeplessness, and general distress and anxiety, without a trace of delirium. In other instances slight illusions prevail without tremor, from which the patient can by an effort arouse himself, and under strong self-directed exertion of the will even command his faculties for a time, and pursue avocations of business, to break down, it may be, hopelessly, a few hours subsequently, if his condition is neglected, misunderstood, or mistreated. Under these circumstances the treatment by capsicum comes in very opportunely, and by its employment we may, as in the case just cited, cut short the disease, and so save the patient from the consequences of his imprudence, and possibly restore him to a reformed life. Another case well illustrates the success of this drug when opium had completely failed to alleviate the symptoms, and seemed on the contrary in many respects to aggravate the patient's condition. The case was that of an individual who had taken six grains of opium within a period of two or three days without sleep being procured, or any relief to the illusions, tremor, and distress under which the patient laboured. After a twenty-grain dose of capsicum in bolus, profound and refreshing sleep for twelve hours was induced, and the patient awoke conscious and restored. In an almost precisely similar instance occurring about the same period a thirty-grain dose of the drug had to be given a second time before full relief was procured. In one or two instances of individuals of confirmed and extremely intemperate habits it was found necessary to repeat the dose some three or four times.

As to the physiological action of the remedy, Dr. Lyons's explanation is that already given in a former communication—namely, that it produces a powerful stimulant and sedative influence by its direct action on the gastric filaments of the vagi. Slight uneasiness in the stomach has been complained of in one instance only after its use, and in two instances somewhat smart purgation was noticed, but without any evidence of intestinal or other irritation.

As at present employed, the drug is administered in bolus made up with honey of roses; but Dr. Lyons suggests the feasibility of its being conveyed to the stomach in the more agreeable form of a capsule.

As capsicum belongs to the great order of the Solanaceæ, Dr. Lyons suggests the possibility of its containing a narcotic principle hitherto undiscovered. He has referred this question for further elucidation to his distinguished friend, Mons. Gages, curator of the Museum of Irish Industry, a chemist of great eminence.

Christison observes, "Capsicum and cayenne pepper belong to the class of irritant poisons; and the latter preparation has been known to cause death. It is entirely destitute of narcotic properties, so far as is known at present. In both respects it constitutes a singular anomaly in the natural order Solanaceæ, which are generally powerful narcotics, but feebly or not at all acrid."

Pepper (Piperaceæ) probably black pepper, was not unknown to the ancients medicinally. Celsus, it may be mentioned, has a chapter headed: "Curatio horroris in febribus si nec balneum quidem profecit, ante accessiornem allium edat aut bibat calidan aquan cum pipere, siquidem ea quoque assumpta calorem movent qui horrorem non admittunt." Dioscorides also alludes distinctly to the use of pepper in curing the shiver of fever, and in later times Van Swieten and Louis Frank have employed it for a like purpose. Under the form of piperin the active principle of black pepper has been by many practitioners in the present century prescribed in the treatment of fevers, some vaunting its efficacy as not second to that of quinine.—*Medical Press and Circular, June* 20, 1866, *p.* 653.

17.—ON THE TREATMENT OF HYPOCHONDRIASIS.

By Dr. C. Handfield Jones, F.R.S., Physician to St. Mary's Hospital.

[We all know how difficult cases of hypochondriasis are to treat —how almost impossible it is to persuade the patient that the alarming apprehensions with which he is haunted have no basis in reality. It does not become a truth-loving and earnest physician to treat such a malady in a superficial manner.]

The following instances illustrate the importance of searching for physical causes which may give rise to the complaint. Romberg (vol. ii., p. 6) says, "The hypochondriasis of students frequently produces palpitation; and an instance of its occurrence from the same cause in advanced life is presented to us in P. Frank himself, who, while devoting special attention to the subject of heart disease in Pavia, when preparing his lectures, was attacked with such severe palpitations, accompanied by an intermittent pulse, that he felt assured he was affected with an aneurism. The symptoms only ceased after the completion of his labours, and after he had enjoyed the relaxation and diversion of a journey." I am well acquainted with the case of a gentleman who, while studying medicine, became haunted by the idea that his abdominal aorta was aneurismally dilated, and was pressing on the bodies of some of the lumbar vertebræ. He was quite aware of the delusive nature of this idea, and very sensibly endeavoured to shake it off by a trip into the country. This, however, entirely failed, and the morbid imagination prevailed to such an extent that his life for some time was rendered very miserable. A man of great mental capacity and attainment assured me that at one time, after much hard work, he became so unnerved that he did not like to drive to the city in his brougham unless his wife went with him. In all these instances the mind was clear and free in itself, and its actings were only impaired by reason of the cerebral machinery in one part being out of gear. To say that these persons were affected with insanity, as the term is currently employed, would no doubt be incorrect; but I cannot avoid believing that such aberrations from the normal acting of nervous centres are minor shades and degrees of the same morbid process which, in its higher developments, deranges the intellectual faculties more or less completely. One cause of exhaustion of nervous power, which often induces hypochondriasis to a most pitiful extent, is spermatorrhœa, or, as it might be better termed, "chronic prostatitis," the result of sexual excesses or abuse. The reality of this cause is admitted by Romberg, Copland, Albers, Trousseau, and Erichsen, although it has been denied by a late writer. I hardly think that anyone who reads the cases recorded by Lallemand, and compares them with those occurring in his own experience, can doubt the pernicious effect of such deep-seated urethral irritation on the superior nervous centres, or the advantage which may result from appropriate local treatment.

The subject of the following history, J. J., a carpenter, aged 37, married, applied to me as an out-patient at St. Mary's, August 6th, 1863. He continued under treatment till Nov. 17th, 1864, when he was discharged fairly well, having improved almost continuously since the beginning of June. He was a rather pallid, pusillanimous-looking individual, tall and spare, who addressed you in a rather anxious, fussy manner, with a low voice. His complaints were of pain in the lower lumbar and sacral regions, extending from the top of the sacrum forwards to a spot near the umbilicus, with weakness of the calves of the legs. At one time he had a sense of rising and suffocation in the throat, or a feeling of weakness in the part, attended with copious expectoration (?) of mucus. A nocturnal urticarious eruption was another of his troubles, but did not last long. His chief affliction, however, was a sense of scalding felt along the urethra after micturition, which lasted about half an hour. With this there was said to be a slight gleety discharge, but of which I could rarely discover any traces. It seemed, however, to dwell much on his mind, and he was constantly referring to it. He attributed it to a previous gonorrhœa, and affirmed that he had never been right since. I tested at one time the effect of a belladonna injection into the urethra, in the hope that it might allay the scalding dysæsthesia, and arrest the discharge. This it might be expected to do, on the view maintained by several eminent authorities, that the drug acts as a tonic to the vaso-motor nerves. In this respect, however, it failed entirely—

indeed, it acted in a contrary way, increasing the discharge considerably, and rendering it more yellow, though the scalding appeared to be relieved. The strength of the injection was fifteen to thirty grains of extract of belladonna, to four ounces of mucilage mixture. I may remark, by the way, that Mr. Jabez Hogg has seen a very weak solution of atropine produce in a healthy eye a very large amount of congestion (*vide* "Ophthalmic Surgery," p. 98). That belladonna acts as a sedative to the sensory nerves of an irritable bladder, or of irritable bronchi, and that in large doses it paralyses the vaso-motor nerves of the head and face, appear to me tolerably well-established facts, and by no means consonant with the views above referred to. I should, however, mention that I have found the drug to act very beneficially in cases of cynanche tonsillaris, and perhaps—as in this instance, the reduction of the inflammatory tumefaction depends probably on arterial constriction—the truth may be, that in large doses it acts paralysingly, in smaller toningly, just, indeed, as alcohol does. Whether a drug can act in one way on one region of the body, and in an opposite way on another, is at present extremely doubtful. But to return to our case. The remedy which appeared to effect a cure was strychnia, which he took in doses of a sixteenth to a twelfth of a grain, together with sulphate of iron and cod-liver oil, during most of the time, for ten months continuously. In another very similar case the same remedy appeared of decided efficacy, but the patient did not attend so perseveringly as the one whose history I have related. In him I think there can be no question that the urethral disorder was slight, and was not the real cause of the depression of nervous power. Had it been otherwise—had the cerebral paresis depended on genito-urinary irritation acting in an inhibitory manner upon the brain, local remedies would in all probability have been required. As it was—as the hypochondriasis was primary, and not of reflex origin, the steady tonic action of a nervine, aided, doubtless, importantly by the cod-liver oil, sufficed to restore the nervous centres to a fairly normal condition.

The following history, may, I think be compared instructively in some respects with the above. A gentleman aged 28, who had been long exposed to the enfeebling influences of a tropical climate, and had suffered several severe attacks of tracheitis, consulted me at first for the latter, but subsequently complained a good deal of what he called a gleety discharge. I found that this was not constant. There was no trace of discharge when I examined the penis. He stated that it came on after any exertion; often occurred quite irregularly, and without any erection taking place. It was evidently more of the nature of an emission than a running, and caused, when it happened, a great sense of debility. He felt it as an inconvenience and a deterioration of his health, but did not complain about it more than a man reasonably might. He showed no signs of hypochondriasis. He acknowledged that his venereal appetite had always been strong, and that he had committed a good deal of excess in this way. I prescribed for him a pill composed of six grains of lupuline, a grain and a half of camphor, and a quarter of a grain of extract of belladonna, two to be taken twice a-day; and in a short time the annoyance entirely ceased. Now I will ask the reader to abserve—(1) That in this case there was quite as much, if not more, genito-urinary disorder than in the first, yet there was no hypochondriasis, which was so marked a symptom in J. J. (2) That the experience of the best authorities is quite decisive as to the production of even actual insanity by genito-urinary irritation. Brown-Séquard mentions having obtained the cure of a case of insanity with general paralysis, in a male who suffered from inflammation of the prepuce and glans with phimosis, by having circumcision performed (*vide* the Lancet, 1861, July 27th). (3) That therefore we must admit that the vital condition of the nervous centres determines very much the effect which shall be produced by a peripheral irritation. A slight one may cause severe symptoms; a greater none at all; according to the amount of resisting capacity possessed by the patient. Lastly, and as the general lesson from the whole, I will remark that it is evidently of the first importance to distinguish primary hypochondriasis *attended* with genito-urinary dysæsthesia from secondary hypochondriasis *depending* on genito-urinary irritation; and that while it would be a great mistake to employ local measures, such as cauterisation, in cases of the first kind, they may be quite necessary in severe cases of the second.—*Lancet, May* 25, 1867, *p.* 627..

18.—CLINICAL REMARKS ON NEURALGIC PAIN OF THE SIDE.

By Dr. PETER EADE, Physician to the Norfolk and Norwich Hospital.

[There is a class of painful local affections in which the complaint is really the expression of an overworked and exhausted condition of the muscles, and of the nerves supplying them. These pains are very common amongst labourers, and are generally supposed by them to be "rheumatism.]

It is seen in the muscles of the arm, chest, back, and especially the legs and the joints of the lower extremities: and whilst it is untouched by the ordinary anti-rheumatic remedies, it readily yields to rest, good diet, vegetable and mineral tonics, and perhaps mechanical support. Patients suffering from this class of ailment are not unfrequently sent into hospital with the odium attaching to them of being malingerers, rather, as it has sometimes seemed to me, because they have shown no marked symptoms of disordered health beyond the pain and weakness they state they suffer from, and because of the inefficacy of the medicines employed to produce a cure, than from any valid reason for such a suspicion. But these patients do obstinately persist in stating themselves to be unable to work, because really they feel themselves unequal to it, and because their muscles, exhausted by daily and too prolonged labour, become really weakened and incapable of further effort, and refuse to do the bidding of the will, because physically unequal to further active exertion.

No doubt real malingerers are met with, but I have seen several examples of men so classed who have gradually and steadily recovered in hospital, and in whom the incapacity to work was unquestionably real, and not the mere effect of a wanting will, and who readily enough resumed their employment when rest and treatment had restored their physical power.

The *actual condition* of the aching nerves in these cases of lateral pain can fortunately only be inferred; for as the affection is one which is rarely fatal, and, indeed, never so, unless some other more serious disease is engrafted upon the disorder which produces it, so opportunities rarely occur of ascertaining their pathological state by post-mortem examination. There is, however, no doubt that the painful peripheral twigs of nerve are, like the rest of the nervous system, in a state of atony; hence arise relaxation of the blood-vessels, congestive fulness, imperfect nutrition, a flabbiness or softness of the nerve-tissue, and then depravation of function, and hyperæsthesia. But as this atonic condition of the nerves is doubtless present more or less in the whole body, as well as in the lower intercostal nerves, the question arises why these nerves suffer more than the others, and why the pain should always be felt in the side even when also present elsewhere. To these questions a very distinct answer cannot perhaps be given, but if we look at the class of affections in which this pain is mostly met with, we shall see that a large majority of them are more or less connected with imperfect uterine action, and although it is quite probable that the disordered uterine function itself may often as well as the pain be merely symptomatic of some more remote cause, yet the connexion between the two is so frequent as to justify their being looked at together.

Many explanations of this lateral pain have been attempted by writers on the subject, who have vaguely and variously referred it directly to sympathy with the uterus, to excitement of the sympathetic system, to general anæmia, &c. Very recently Dr. Martyn (British Medical Journal, 1864) has endeavoured to show that it is a reflex neuralgia, induced by, and expressive of, some distress in the heart; and he gives details to show how such a causation might be anatomically probable. But my observations tend rather to show that a true neuralgia of the heart does exist which is quite a distinct affection from the one we are considering, and that with this cardiac neuralgia, whilst there is pain distinctly referred to the heart, infra-mammary pain is rather the exception than the rule; and that, on the other hand, with uterine or other constitutional irritation, it is almost universally present. Moreover, such an origin could hardly account for the widely diffused pain and tenderness so often felt not only along the side, but over a large portion of the dorsal spinal region. Looking, therefore, to its close connexion with, and almost constant presence in, uterine and ovarian disorders, and its relation to those faults of the blood which so continually eventuate in them, and bearing in mind also the light which the researches of Drs. Marshall Hall and Brown-Séquard have thrown upon reflected nervous affections, I am more inclined to the opinion expressed by the late Dr. Todd in his clinical lectures, and by others, that this pain is virtually merely the reflected expression of an uncomfortable and irritated uterus or ovary, the irritation of which, passing along the hypogastric plexus and sacral nerve to the spinal cord, is then reflected along it to one or more of the intercostal nerves, in which, as is usual, the depraved sensation is felt at the peripheral extremity. This would thus form a generative sympathetic arc or circle, with one extremity formed by the peripheræ of the uterine nerves, the other by those of the intercostals; the fact that the pain of the back when present is rarely felt lower than the ninth or tenth dorsal vertebra being explained by our knowledge of the fact that the spinal cord usually ends at about the level of the tenth or eleventh vertebra, and divides here into the nervous cord forming the *cauda equina*—in other words, it ceases here to be a sensitive or sensory nervous centre, and becomes only a conducting body. Why the pain should generally be felt in one side only, and that the left, is a problem perhaps yet unsolved, although the nervous relations in the chest described by Dr. Martyn may perhaps be the real anatomical cause of it; and I can see no reason why his explanation of the localisation of the pain through the peculiar relations and distribution of the sympathetic and intercostal nerves should not be equally true, whether the exciting eccentric cause be seated in the thoracic or the pelvic cavity. I have tried, but failed, to discover any greater tenderness (when any exists) in one ovary or side of the uterus than the other—a tenderness which, on Brown-Sequard's theory of the decussation of the sensitive fibres in the spinal cord, should be looked for usually in the right rather than in the left flank.

It is remarkable that whilst leucorrhœa (the connexion with which was especially pointed out by Dr. Todd) and other chronic uterine and ovarian affections induce this pain, that gonorrhœa, also a disorder accompanied with constant discharge, but which is a vaginal and not a uterine affection, does not give rise to it; and the same is the case with chronic catarrhal and other disorders of the bladder. When any secondary nervous symptoms do result from these causes, they are observed not in the upper part of the body, but in the lower extremities, the lesion being paraplegic, and the reflex action being then induced in another arc, with the nervous circle composing it on a lower level of the body, and the direction of the nervous current reversed.

It is also to be noticed that men who spend their days in the most sedentary occupations, and often in a most confined atmosphere, do not suffer from this pain of the side as women so confined and so occupied do. Shoemakers, for example, suffer much from epigastric sinking and uneasiness, irregular action of the heart, and hypochondriasis; but, unless other causes are superadded, they do not suffer from disorder of their generative organs; nor do they complain of intercostal pain or tenderness, nor yet spinal symptoms. Tailors, again, who often work under conditions not very dissimilar from those of milliners and dressmakers, and often get almost similarly ætiolated, do not suffer from this form of pain as these latter do. It is necessary, therefore, to look for some other cause as active in producing it, and there seems little reason to doubt that the uterine nervous system in a state of continued irritation is this cause.

But, although this pain is so unfrequently met with in the male sex, yet instances are occasionally seen of its occurrence in them, and I have met with several examples of male patients who have complained of abiding pain in one or other side not to be distinguished from that seen in women. Such patients are generally of a nervous, excitable, or, indeed, of truly hysterical temperament, whose emotional system has been excited unduly and beyond its power of resistance. A somewhat similar pain is also occasionally met with in weakly men who have exhausted the nervous energy of their intercostal muscles by too great or too prolonged muscular exertion of the chest, and is notably seen in clergymen, public speakers, and others, who make great efforts in the public exercise of their vocation.

Neuralgic pain of the side is but a symptom of disease, and not only so, but it is a symptom which may arise in the course of many different, though perhaps allied, diseases. To speak exhaustingly, therefore, of the remedies proper for this pain would be to compose a treatise upon those means, mental, moral, hygienic, and medicinal, which are appropriate to the numerous conditions of disordered health in which it occurs. This is not the object of these remarks, and I will only observe that a glance at the list of causes, and of the morbid affections with which it is associated, will suggest a large proportion of the means essential for their and its removal. Thus, according to the special circumstances of the case, a good or improved diet, change of scene, sunlight, cold sponging or bathing, shower-baths, frictions to the skin, daily and appropriate walking exercise in the open air or on horseback, cheerful society and occupation, the avoidance of close and heated rooms and of late hours, or of too much emotional excitement, with means directed to the quieting and soothing of an over-wrought body or mind, may all in their proper place conduce to recovery.

So, again, some one or more of the various nervine tonics or alteratives may contribute to its removal by restoration of the general or uterine health. Such are ammonia, valerian, assafœtida, camphor, steel, quinine, strychnine, acids and alkalies, iodine, bromine, cod-liver oil, &c., &c. Vaginal or uterine injectines, or mechanical appliances, will also in appropriate cases be useful. Locally, more or less relief will be obtainable from the judicious use of warm fomentations, either simple or medicated ; of belladonna or opium plasters ; of liniments of opium, belladonna, aconite, or chloroform ; of epithems or blisters of chloroform ; and occasionally of blisters, sinapisms, leeches, or ice-bags. Belts or bandages to the side and mechanical supports to the spine will also be occasionally of service.

My own special experience of the treatment of the disorders in which this pain occurs, and therefore of the pain itself, may be summed up as follows :—That steel is usually essential (except there be pure active hysteria with plethora, or acute suppression of the menses in otherwise healthy subjects), and that its efficacy is much increased by the addition of a little aromatic spirit of ammonia, or of what is often better, the ammoniated tincture of valerian.

That where there is obstinate amenorrhœa, the addition of a grain or two of iodide of potassium to each dose of the steel mixture is often extremely beneficial.

That where ovarian excitement predominates, camphor and henbane in pills, bromide of potassium with steel and velerian, and chloroform blisters, or perhaps leeches to the flanks, will be found most useful, and that where, with general debility, leucorrhœa is the existing form of uterine disorder, a combination which I have used largely in private as well as hospital and dispensary practice will often be found singularly efficacious. The following is my formula for this chalybeate mixture :—℞. Tinct. ferri sesquichlor. ♏xv. ; acid. nitric. dil. ♏xv. ; magnes. sulph. ʒss. ; tinct. aurantii ♏xv. ; aquæ ad ℥j. M. ft. haustus bis vel ter in die sumendus. When much weakness is present, a little quinnine or tincture of nux vomica is a useful addition to the draught. The *rationale* of its beneficial action would appear to be that in many of these cases there is a sluggish state of liver, which increases the passive retardation of the abdominal circulation that already exists from the very nature of the affection ; that the combined effect of the sulphate of magnesia and nitric acid is to disgorge the liver of its bilious matters, and so to relieve its general and portal congestion, and by its secondary action on the bowels to stimulate alvine secretion as well as evacuation, at the same time that, by thus relieving the portal as well as general abdominal circulation, it enables the pelvic veins more readily to empty themselves, and thus to diminish the passive uterine and ovarian congestion which doubtless is often also present in these cases, the steel and other tonics further improving the state of the blood, and so secondarily giving tone to the nervous centres and to the uterine nerves and fibres. But whether or no this be the proper explanation of its action, certain it is that it is a most useful medicinal combination, and often does great good where steel alone or in other forms has been given in vain. Its mode of action must be somewhat that of the morning dose of sulphate of iron and sulphate of magnesia recommended by the late Dr. Rigby, but it has the advantage over it of being less unpleasant, less voluminous, and, being given in smaller doses, of acting more gradually. It is also an equivalent, but in my opinion far preferable, to the combination of sulphate of iron and sulphate of magnesia in common use in the London Hospitals.

Locally, leeches (when applied either to the side or to the spine) often relieve at first, but the relief given is seldom permanent, and they frequently leave a disagreeable sensation of weakness, but they are sometimes distinctly of service when applied over the ovaries or in the groins. Sinapisms and blisters are often useful, but, in my experience, are more efficacious at a later period, when steel and other remedies have been for some time persevered with, and then a blister will sometimes at once and completely remove the pain.

Belladonna plasters worn continually on the painful side or in strips down the back are often of much use ; opium or belladonna liniments are less to be relied on. Chloroform, applied as a rubefacient or as a blister, sometimes relieves markedly, and hot poppy fomentations will often prove of great advantage when applied for some length of time in cases where the pain is severe or shooting in character. When the spine is a principal seat of pain, and the patient suffers from undoubted spinal weakness, a mechanical support, after the fashion of the instrument made by Pratt or of that recommended by Dr. Dick, may sometimes be of great service. It only remains to allude to the employment of subcutaneous injections of morphia or belladonna, or of the local etherisation of the aching part by Dr. Richardson's apparatus in those rare cases in which the pain is sufficiently severe to necessitate the having recourse to more potent sedatives or anæsthetics.—*Medical Times and Gazette, July* 20, 1867, *p.* 64.

19.—ON NEURALGIA.

By Prof. Trousseau.

In his lecture on neuralgia, Professor Trousseau brings forward his important observation that in all cases of this disease there is more or less acute pain on pressure over the spinous processes of those vertebræ which correspond to the origin or point of exit of the affected nerves ; and to this *spinous* point he justly attaches very considerable diagnostic value. He draws attention to another peculiarity, concerning which, he thinks, writers have not been sufficiently explicit—the existence, namely, of cutaneous hyperæsthesia at the points of exit of the nerve-trunks. This he terms the *spot of peripheral expansion.* He denies the statement of Valleix regarding the *superficial tender spots* to be found in intercostal neuralgia, but admits the existence (amongst others) of those which he indicated in cases where the cranial nerves are affected. Where the neuralgia is superficial, Trousseau finds that the local application of atropine or belladonna is sufficient to relieve pain in the majority of cases. He generally employs a compress steeped in a solution of sulphate of atropine (five grains of sulphate of atropine in three ounces of distilled water), and covered with a piece of oiled silk to prevent evaporation. This application is continued for an hour at a time, and is frequently renewed, provided no disagreeable constitutional effects are produced. When the neuralgia is more deep-seated or severe, he has recourse to the endermic use of morphia, or the subcutaneous injection of morphia or atropine ; and in obstinate cases he makes an incision through the skin, and places in the wound one or two medicated boluses. When he adopts the endermic method, he removes the cuticle by means of ammonia, as it can be done in this way much more neatly and expeditiously than by means of cantharides. He also advocates the inhalation of chloroform during the attacks.—*Lancet, July* 6, 1867, *p.* 12.

DISEASES OF THE ORGANS OF CIRCULATION.

20.—ON CARDIAC DIAGNOSIS APART FROM MURMURS, AND SPECIAL STATES OF VALVULAR DISEASE.

By Dr. W. T. Gairdner, Professor of Practice of Physic in the University of Glasgow.

[In all cases where the state of the heart is in question much may be learnt by attention to little things. The frequency and character of the pulse, the state of the radial artery, and of other arteries, as of the temples and of the superficial part of the neck ; the situation and character of the impulse against the chest is also of importance. The frequency and character of the respiration must also be noticed and compared with the frequency and character of the pulse. The nominal relation is about 1 to 4, and if we find this much altered we should ascertain the reason.]

These are a few of the preliminary points, which a physician of observant habits and good education will accustom himself to note in almost every case of suspected cardiac disease. I now pass to some observations touching more directly the physical state of the heart itself.

The heart lies beneath and to the left of the sternum, inclining, as you are well aware, from right to left, and from above downwards; so that while the base of the ventricles may be said to be about the left border of the sternum, near its junction with the third left costal cartilage, the apex extends to a point at a variable distance below the nipple, and usually corresponding with the interval between the fifth and sixth ribs. Between these limits lies the extreme length of the ventricles ; and the breadth of the ventricles may be roughly estimated in a normal subject as extending from the middle line of the sternum to a point indicated either by the apex-beat, or by the extreme limit of the percussion dulness leftwards. But in estimating the position of the heart you have always to keep in mind that a considerable portion of the organ is not in contact with, nor even very near the wall of the chest. Both the auricles entirely, and the left ventricle in great part, are concealed as it were from direct observation by the lungs, which overlap the base of the organ, and by the right ventricle, which lies in front, and thus interposed between the left ventricle and the ribs. The small portion of the left ventricle which constitutes the apex of the heart in normal subjects, and the impulse of which at the fifth intercostal space constitutes the apex-beat, is literally the only part of the whole left heart that is directly within the scope of observation from without. The right ventricle, on the other hand, with the pericardium alone intervening, may be said to be almost in contact with the thoracic wall over the greater part of its surface. Now, upon this arrangement of the parts depends a very important point in diagnosis. Normally, you have the left ventricle, through its extreme apex only, impinging with an extremely well-marked abrupt impulse on a particular point in the wall of the chest, and thus giving rise to what we call the apex-beat But the greater part of the left ventricle retires altogether from observation, while the right ventricle, with its much larger surface of contact, and its much more diffused and comparatively feeble impulse, occupies the greater part of the precordial space. This is the normal condition: a direct, well-defined, punctuate impulse of the left ventricle; and an equally direct, but much more diffused impulse of the right ventricle. Suppose, however, that you have one of these ventricles dilated or hypertrophied as compared with the other; a certain amount of hypertrophy of the left ventricles will simply lead to increase impulse, and at the same time to displacement downwards, of the apex-beat. A much greater degree of hypertrophy, and more especially of dilatation of the left ventricle will, it is true, lead to modifications of a quite different kind, hereafter to be described; but the effect of hypertrophy of the left ventricle upon the apex-beat is usually to intensify it, and perhaps also to increase its diffusive force in accordance with the increased power of the ventricle. In the case of the right ventricle, on the other hand, a very moderate amount of hypertrophy or dilatation has usually a most remarkable effect upon the apex-beat. Dilatation of the right ventricle, in fact, to the extent to which it renders that ventricle more prominent, also causes the left ventricle to recede from the surface; interposes, in fact, the right ventricle as a buffer (so to speak) between the impulse of the left ventricle and the thoracic wall, and thus to a certain extent tends to suppress the proper apex-beat. In other words, while up to a certain point hypertrophy of the left ventricle usually tends to increase the distinctness and definition of the apex-beat, hypertrophy of the right ventricle to a corresponding extent almost always tends to the diminished distinctness of the apex-beat, and ultimately to its extinction. Now partly by discovering and carefully defining the apex-beat; partly by listening to the sounds and closely observing the impulse all over the heart's surface; partly also by the use of percussion, we are able to discover with tolerable certainty what is the state of any particular case as regards the *presentation* (to use a term familiar in midwifery), of the right or left ventricle, that is, we are able to determine, up to a certain point, whether the right or the left ventricle is the more prominent, and whether it owes this prominence to hypertrophy, or dilatation, or both. I have here stated in general terms a very important diagnostic problem, on the details of which it will be necessary to make some further remarks.

The first step in the solution of this problem is to determine, if you can, the exact position of the apex-beat. What we call the apex-beat, proper, is an impulse of a quite peculiar and definite character, tangible, or visible, or both, limited to a single spot you can cover with the point of one finger, or at most with the point of two fingers placed in a line in the intercostal space. If the impulse is over a much greater space than this, or if it is not perfectly well defined within these limits, you have reason to suspect that it may not be the apex-beat proper, or that the anatomical conditions ruling the apex-beat are considerably modified. In every instance, therefore, of suspected cardiac disorder, we feel for the apex-beat; we determine as far as possible its character and position, and reason from these to other data connected with the impulse and sounds of the heart. We expect to find the apex-beat, as a rule, in the fifth intercostal space, about half-an-inch (perhaps more, perhaps less), within a vertical line falling from the nipple. And if the apex-beat is not found in this, its normal position, we expect before we have done to know the reason why.

It is necessary here to observe, however, that in certain cases, not very easily defined or classified, you may have great difficulty in finding the apex-beat at all. The action of the heart may be too feeble, or the patient may be too fat and flabby, or some other morbid conditions may have intervened to alter the relations of the normal apex-beat to the chest-wall. The position of the apex-beat, moreover, is normally, to some extent, variable, even in the same subject, according to position; and it varies very considerably in its relation to the chest-wall in different subjects, within the limits of health, the chief force of the impulse being sometimes received on a rib, but more commonly in the fifth intercostal space, as above stated. When you are in any difficulty on this point, lay the patient over on the left side, well towards the face, and feel with great care for the apex-beat in the fifth and the adjoining intercostal spaces. If you cannot detect the defined and strictly localised impulse already described in this position of the body, you will probably not be able to find it at all; and the inference then will be, that there is something in the condition of the heart and great vessels which prevents the apex of the left ventricle from coming to the surface.

Of course you will take care not to be misled by displacement of the heart; from pleuritic effusion, which may carry the apex-beat over to the other side of the chest altogether; or, from atrophy of the upper lobe of the left lung, which may, (as I have elsewhere shown) carry the apparent apex-beat up to the second and third intercostal space. It will be well, therefore, in all cases in which the apex-beat cannot be distinctly verified at first, in the position above described, to try with the stethoscope if you can find a spot, either at the usual seat of the impulse, or, if not there, at any other point, however remote, at which the first sound of the heart is decidedly and definitely more clear and superficial than elsewhere; then resume the examination by touch, and try if you cannot discover there a tangible, and perhaps a visible, apex-beat—not the heaving of a comparatively large space, but a defined and punctuate impulse, as aforesaid.

Next, if you find ever so little trace of an apex-beat, examine the radial pulse, or the pulses in the other arteries, and compare, mentally, the strength of impulse denoted by the arterial pulse with the force and definition of the apex-beat; for I need hardly say that there is an obvious reason why there should be a certain correspondence between the one and the other. If you should find a very strong and well-defined apex-beat, for example, with a very feeble arterial pulse; or if, on the other hand, there should be found a very strong, or even a normal, radial pulse, associated with a very feeble apex-beat; in either case there would be reason to enquire into the cause of the discrepancy, and much more so if, with a strong or normal radial pulse, you should fail in discovering the site of the impulse of the left ventricle at all.

Now, suppose you have arrived at this point—the impulse of the left ventricle at the apex is either undiscoverable or abnormally indistinct, as compared with the radial pulse—you desire to know what is the cause of the discrepancy, the cause of the relatively feeble or perhaps inappreciable character of the apex-beat. You have as yet simply noted a fact which may have many causes, and you are advancing step by step towards the true cause. It may be merely that the apex beats upon a rib, instead of into an intercostal space; in such a case you will have, of course, a very ill-defined apex-beat, but then the stethoscope will inform you of a clearly defined and circumscribed space corresponding to the apex-beat in which the first sound has its usual well defined superficial character, and this will save you from error. Again, an emphysematous lung may, by projecting over the apex, *i.e.* over the small portion of the left ventricle which is normally uncovered by lung, suppress the apex-beat; but here the signs and the history of emphysema of the lung, and especially the state of the percussion near the edge of the heart, will usually reveal the cause of the difficulty. Again, the dilatation of the left ventricle itself may render the apex-beat indistinct; not (I desire you to remark) hypertrophy merely, but either dilatation alone, or dilatation with hypertrophy of the left ventricle. Hypertrophy alone, or hypertrophy with moderate dilatation, does not suppress the apex-beat, but rather increases its force, and within certain limits its definition, as I have already said; displacing it at the same time towards the left and downward. But great dilatation, apart from hypertrophy, of the left ventricle, will, by rounding the apex, by weakening the fibre of the heart, and by increasing the extent of surface impinging upon the chest, have the effect of relatively weakening, and possibly of apparently suppressing, the apex-beat. Effusion into the pericardium will also have the effect of suppressing the apex-beat; and lastly, as I formerly remarked, the mere distension of the right ventricle with blood, and, still more, the permanent hypertrophy and dilatation of the right ventricle carried to a certain point, will cause the apex-beat of the left ventricle to disappear altogether.

Before proceeding further, let me state to you a remarkable fact, or rather an experiment which any of you may perform upon himself, and which completely illustrates, in a manner that you will not easily forget, this influence of the state of distention of the right ventricle on the apex-beat. Put your hand upon the fifth intercostal space, lean over well to the left, and make sure that you have clearly defined the apex-beat in your own person. You will find that you can suppress it, or nearly suppress it, at will, simply by holding the breath. Do not take a very deep breath, for that is to cause the lung to expand, and so to project more or less over the apex, but simply *stop* breathing, when you have your hand upon the apex-beat, and when you have made sure that it is quite well defined. Keep the same position without breathing as long as you can, until, in fact, the sensation becomes intolerable, and you will find that after a short interval the impulse of the left ventricle becomes less and less distinct, and finally, but quite gradually, it vanishes altogether. What is the cause of this? Not, of course, that the circulation has ceased, for you will still find the pulse quite distinct at the wrist, but simply because all the time that you have been holding your breath the right ventricle has been filling and over-filling, so that long before you are even half asphyxiated the right ventricle and auricle have been becoming more and more distended—more and more prominent; and thus the left ventricle, or rather the apex of the left ventricle, has been to a corresponding degree thrust into the back ground. This is a most instructive experiment, which I was led to make very frequently while examining into the case of M. Groux, the singular example of congenital cleavage of the sternum—a case which illustrated so many points in the physiology of the heart. You will learn from this experiment, which you may easily repeat as often as you please without danger, almost without inconvenience, precisely what happens in nearly the whole of the cyanotic class of diseases, and in all diseases whatever which are attended by well-marked hypertrophy and dilatation of the right side of the heart.

The actual tests, then, of hypertrophy, or dilatation of the right ventricle, are these:—With a suppressed or a relatively diminished left apex-beat, you have a diffused impulse to the right of the usual position of the apex-beat, and commonly easily distinguished from the latter (even when both are present) by its diffused and heaving character, readily appreciated by the palm of the hand strongly pressed over the costal cartilages. Sometimes you will have no difficulty in detecting a proper apex-beat in its usual situation, or perhaps in such a position as to indicate hypertrophy of the left ventricle, and, in addition, you will discover the diffused flat impulse, heaving up the whole palm of the hand, quite distinguishable from the other, and extending from the third or fourth left cartilage almost to the xiphoid cartilage, or into the epigastrium. In such a case it is to be presumed that both ventricles are very considerably enlarged; but the relative degree of enlargement of the right or left ventricle will be determined mainly by the degree to which the apex-beat retains or loses its definite character.

[To aid in the diagnosis the limits and position of the heart must be approximately determined by percussion. There is no difficulty in defining the position of the left ventricle by the apex-beat, and the right border of the organ can be determined by percussion. The right border, except in cases of dilatation and hypertrophy of the right ventricle, usually corresponds with the mesial line.]

If the apex-beat is not distinct and punctuate, you are left in uncertainty as to the amount and the limits of the precordial dulness which corresponds to the left ventricle, and there is no way, so far as my experience goes, in which in such cases the precise limit of the two ventricles can be determined. But, speaking generally, if you find even the trace of a punctuate apex-beat, you may assume that by far the greater part of the dull space which is to the right of the apex-beat and above it corresponds to the right ventricle, and when the apex-beat cannot be defined, you may generally assume that nearly the whole of the precordial dulness corresponds to the right ventricle, or perhaps also the right auricle ; at all events, to the right side of the heart. Now, if this space gives to the hand a dull, flat heaving impulse, extending towards the xiphoid cartilage or into the epigastrium, and if the sounds of the heart are propagated with undue distinctness in the same direction, while they are extremely indistinct, muffled, or distant over the usual site of the apex-beat, you may feel perfectly satisfied that the right ventricle and auricle are in a state of comparative dilatation and hypertrophy, from some cause or other, most probably from emphysema of the lungs.

But there is yet another test of the comparative condition of the two ventricles, viz., the state of the second sound of the heart. You know that the second sound is caused by the blood falling back upon and closing the sigmoid valves. Each ventricle, in contracting and expelling its blood gives rise to some part of what is known as the *first* sound of the heart, the precise origin of which has been the subject of much discussion ; but there is no such discussion as to the origin of the *second* sound, which is all but universally admitted to arise from the recoil of the blood in the arteries upon the sigmoid valves, at the moment after the systole of the ventricles is completed, and when their diastolic movement is about to begin. Now it is important to remark, that although they are often blended to the ear, each ventricle has a quite separate second sound of its own, corresponding with the recoil of the blood in the one case of the pulmonary artery, in the other of the aorta, on their respective sigmoid valves. And although the observation is at times a difficult one, and cannot be followed out in any case without great caution and special experience, it is certain that the sound of the pulmonic recoil can at times be accurately distinguished from that of the aortic recoil, and *vice versa.* To acquire this special skill it is necessary that your ear should be familiar with the precise character of the sound peculiar to each ventricle, normally. To obtain the aortic second sound in its purest form, listen in a normal chest exactly over the second right costal cartilage, and very close to the sternum—indeed with the stethoscope overlapping the edge of the sternum as well as that of the cartilage. To obtain the pulmonic second sound as much as possible isolated from the other, listen over the third left costal cartilage near the sternum, but not quite so near as in the other case. There are various circumstances, which can only be learned by experience, which tend in certain cases to modify the results of this examination, and the position of these sounds ;

but, speaking generally, you may say that the aortic second sound corresponds with the point at which the aorta emerges from below the pulmonary artery, and approaches the surface; while the pulmonic second sound corresponds in position with the extreme base of the heart, where it is uncovered by lung, or in certain cases with the *conus arteriosus*, or the most prominent part of the right ventricle. In cases where, from particular circumstances, the difficulty of distinction is great, the aortic second sound may be distinguished absolutely by placing the stethoscope over the innominate artery, or the arteries of the neck; while the pulmonic second sound may be heard at the position before indicated, so that, in particular instances, a thoroughly skilled observer will find himself able to declare with positive certainty that one or other of these sounds is relatively exaggerated, or deepened in tone, or otherwise altered in character.

Now, in these alterations of the second sound we have a very important aid to our estimate of the state of the two ventricles—an auscultatory test, as it were, not perhaps in all instances to be trusted alone, but acting precisely like many of our most useful diagnostic tests, in confirmation or correction of others. The quantity of blood expelled by each ventricle at each systole—the force and rapidity of the systolic movement—the elasticity of the arterial walls—the amount and rapidity of their distention—the suddenness and force of the recoil, are all factors which enter into the determination of the precise character of the second sound in each case. And amid these complicated conditions the ear of the skilful observer has to decide and define as it may. But if either the left or the right ventricle is, to a very marked extent, predominating in power, it will probably expel a larger quantity of blood at a higher rate of pressure, and the recoil will be correspondingly sudden and strong. Accordingly, there will be an increased, and to some extent an altered, character of the second sound which is peculiar to that ventricle and its corresponding artery. If, on the other hand, there is a marked deficiency in the power or the expulsive activity of either ventricle, or if the mechanical conditions under which it expels its blood are interfered with, there will be a corresponding diminution or *tonelessness* of the second sound corresponding to that ventricle. In the case of the pulmonic second sound, the increase of the recoil which attends hypertrophy of the ventricle is very frequently observed, and not unfrequently in connection with an equally marked diminution in the tone of the aortic second sound. When this is the case the conclusion as to the state of the two ventricles is irresistible. On the other hand, the aortic second sound is often so peculiarly altered as to lead to results of great importance in the diagnosis of the condition of the left ventricle and of the arterial system, but these results will come under consideration on a future occasion.—*Glasgow Medical Journal, July* 1867, *p.* 81.

21.—THE SOUNDS OF THE HEART IN THEIR RELATION TO PATHOLOGY.

By Dr. Arthur Leared.

(Read before the Medical Society of London.)

Dr. Leared commenced by stating that the normal and abnormal sounds formed by the circulation of the blood are more closely allied in their causes than is generally supposed. He added that it would be granted that whatever explanation of the normal sounds fits into and accords most clearly and in the larger number of instances with the explanation of their abnormal changes, is most worthy of acceptance; and, conversely, that any theory which is incapable of adaptation in a salient respect should be rejected. Most theorists have been compelled to ignore any connexion between the two classes of sounds, on account of incompatibility in their explanation of the respective causes. There are certain broad facts in which, if physiology and pathology do not agree, there must be an absence of truth on one side or on the other. *De minimis lex non curat*; and he proposed, as sufficient for his purpose, to deal with the more important points of cardiac diagnosis, and to test the truth of prevailing theories of the sounds of the heart by investigating the connexion which exists between the sounds issuing from the organ in health and in disease. He affirmed that the sounds of the heart, in common with all the sounds formed by the circulation, mainly emanate from the blood itself. They are not essentially due to the vibration of valves, or to the tension of muscles; and even if these causes contribute in a minor degree to form the sounds, it does not affect the argument If the blood circulated without any rhythmic motion, no sound would be caused; but it is thown into broken and impetuous currents by the action of the heart, and sounds are thereby necessarily produced. Besides rhythmic motions, certain conditions are requisite to produce the normal heart sounds. If all these conditions be not present, the sounds will be abnormal. Both sounds of the heart are formed by the blood itself in the commencement of the aorta and pulmonary artery, and are closely allied in their mechanism. Dr. Leared in part quoted from and in part referred to his published writings on this subject. The first sound he maintained to be caused by the blood in the ventricles coming into forcible contact with that in the great vessels, which had just attained a state of momentary repose. The second sound he alleged to be due to falling back of the blood in the outlet vessels, and its arrest by the valves—not, however, by the vibration of the valves, but on the same principle as the sound is formed in a tube through which water is flowing from a cistern, when its flow is suddenly arrested by turning a cock. Four conditions are necessary for the production of the perfect first sound. 1. Sufficient viscidity of the blood. 2. Sufficient pressure upon the blood. 3. Sufficient force in the circulation. 4. The absence of obstruction at the outlets of the heart. If the first condition—viscidity—be wanting, the blowing sound so often heard in anæmia is substituted. If the second be deficient, the first sound also changes to a murmur, as happens in cases of great hemorrhage, by which the pressure on the remaining blood is much diminished. If the third be deficient, the first sound simply becomes proportionately faint, as proved by the want of sound in fevers when the power of the heart is much impaired. The fourth condition is resolvable into the second, because any obstruction at the mouth of the aorta or pulmonary artery must cause diminished pressure beyond the obstruction. For the production of the second sound less force is requisite, and for this reason, when the heart becomes feeble, the first sound becomes impaired much sooner than the second. Viscidity is not a necessary condition ; hence in anæmia the second sound never degenerates into murmur like the first. Murmurs in the heart are nothing more than modified shock or normal sounds, and in their causes all the sounds formed by the circulation of the blood are closely correlated. And since certain conditions are requisite for the formation of a normal or perfect sound, Dr. Leared proposes the term "degenerate" to express any imperfection which would constitute it an abnormal sound. Dr. Leared next dwelt upon an inaccuracy of expression used by medical authors. Speaking of murmurs in the heart, they commonly say, "there was a *bruit de soufflet* with the first or the second sound," as the case may be. This arises out of the false conclusion that normal sounds have their origin in something different from that which produces murmurs. But in that case a sound emanating from anything but the blood itself, whether muscle or valve, would invariably be heard coincidently with a murmur—in other words, with an admitted blood sound—except when the murmur is loud enough to drown the other sound—a rare occurrence. When the subject is thus approached, an incongruity is perceived, and, to elude this, the word "with" when it should be "instead of," is employed. For the normal sounds are not generally acccompanied by, but substituted by murmurs. In most cases in which simple murmurs are heard immediately over the left side of the heart, corresponding faint normal sounds may be heard by shifting the position of the stethoscope slightly, so as to place it over the right side of the heart. In considering the question whether a normal sound is supplanted or merely overpowered by the loudness of the murmur, the duplicate origin of the sounds must be kept in view. In making an examination the stethoscope must be fairly placed over the cavity from which the murmur proceeds, and not at the same time partly over the adjoining cavity. Dr. Leared maintained that other changes—besides that of the first sound into murmur—as of its intensity, duration, and pitch, are better accounted for by the present explanation than by any other. They are due to variations in the force with which the blood is impelled, or else in its pressure or viscidity; while by no other means can the complete degeneration of the sound into a murmur of moderate loudness be satisfactorily explained. Viscidity of the blood has been stated to be one of the essentials for the formation of the normal first sound, and, what is very significant, not of the second sound. If water instead of blood circulated in the body, no such sound would ever be heard. The first sound would always be a murmur, while the second would be the same as if caused by blood. It is on this principle that the first sound so commonly degenerates into a murmur in anæmia. The second sound, however, never in this disease undergoes the same change, because at the time of its formation the blood in the great outlet vessels is separated from that in the ventricles by closed valves. The formation of sonorous currents in the intermingling blood owing to its increased mobility is thus prevented. Dr. Leared added, when the advocates of other theories of the normal sounds of the heart afford by them an intelligible explanation of these everyday facts—assuming the heart and its great vessels to be healthy, the blood being less viscid than natural, the first sound is prone to change into a murmur, while the second sound never so changes—he will be prepared to question the truth of the present theory. Increase of viscidity of the blood is also probably attended by modification of the first sound, and the result of experiments out of the body shows that this would tend to make the sound more prolonged and less loud than natural. In typhus fever fibrin exists in excess in the blood, and the impairment of the first sound of the heart so often observed in this disease may be in part due to this cause. Diminished pressure, by which is implied that the total quantity of blood in the heart and arteries is less than natural, also causes the first sound to degenerate. Sufficient friction to induce murmur is then also formed between the blood issuing in a less body than natural from the ventricles and the blood in the great vessels. Nothing is more conclusive of this position than the fact that one effect of excessive hemorrhage is to transform the first sound into a murmur. Dr. Marshall Hall's experiments proved that this transformation occurs in dogs, and it may be sometimes observed in the human subject, as in great loss of blood after parturition. Impairment in the quantity of blood, then, produces the same result as impairment of its quality; and Dr. Leared again challenges the advocates of the origin of the first sound from the structures of the heart itself to explain by their theories the change, in this instance, of the normal sound into a low-pitched murmur. In certain cases murmurs are heard equally loud at the base and at the apex of the heart in which, after death, the mitral valve is alone found diseased; whereas, according to theory, the aortic orifice ought to be also diseased. Dr. Leared thinks this may be explained by the diminished volume of blood sent by the ventricle into the aorta in consequence of mitral regurgitation. By this means the aorta would be, in relation to the blood sent into it, in the same position as if a great loss of blood by hemorrhage had occurred. Thus the sound would degenerate into a murmur on the principles explained, and a double murmur, one being regurgitant and the other direct, would ensue. Sufficient force in the circulation is so necessary a condition that whatever theory of the sound is adopted, it must always be a constant factor. Loss of force implies loss of velocity in the blood, but this does not cause degeneration of the first sound: it merely becomes less loud, as in the case of typhus fever. Absence of obstruction at the outlets of the great vessels is the last condition to be considered. This condition is closely allied to one previously considered—namely, that there shall be sufficient pressure on the blood, for an obstruction from atheroma or contraction of the vessel produces diminished pressure beyond it, and then currents are permitted by which the integrity of the shock or natural sound is destroyed. Conversely, any considerable dilatation of the aorta is a cause of lessened pressure, because the proper relation between the blood which it contains and that which it receives from the ventricle is altered. Hence dilatation is a not unfrequent cause of murmur. The production of murmur from diminished pressure is easily illustrated by experiment. Let one end of an india-rubber tube of an inch diameter, and of moderate length, be connected in a perpendicular manner with a cistern containing water; and let the other end be also inserted into a vessel of water. As soon as any air that may have been contained in the tube has been expelled, the water will flow through it quite noiselessly. If the tube be now somewhat compressed in the middle, and the ear be applied below the point of compression, a murmur will be heard. It will be found also that the murmur is louder here than at any other part of the tube, and therefore formed in this position. But if the murmur be not due to vibration of the compressed part of the tube, why should it be thus localised? Because the velocity of the water as it passes through the obstruction is proportionate to the pressure upon it—in other words, to the length of the column above it. Here it must be explained that although, if the water be at rest, the pressure upon it would go on increasing in the ratio of the length of the tube, notwithstanding the obstruction, the case is different when the water is in motion. Now, if a tube be obstructed to the extent of half its area, it is manifest that, since it is also full below the obstruction, the more rapidly moving water from above will be driven forcibly through the more sluggish stream below. And at the spot where this first occurs—namely, immediately below the obstruction—the murmur will be most intense; for as this point is receded from, the velocity of the two streams becomes equalised, and the friction between them, which is the cause of sound, vanishes. Doubling or reduplication of the heart's sounds is an obscure phenomenon, of which no satisfactory explanation has been as yet offered. Dr. Leared detailed the various explanations that have been given, together with the objections to them. He has often noticed that when a tap from which water is flowing is suddenly turned, the jarring noise which results is not merely single, as previously described, but sometimes double, from a rebound of the descending fluid and a second concussion. He thinks it possible, therefore, that reduplication of the second sound of the heart may be caused in this way.—*Medical Times and Gazette, Aug.* 31, 1867, *p.* 241.

22.—ON THE USE OF NITRITE OF AMYL IN ANGINA PECTORIS.

By Dr. T. Lauder Brunton, Senior President of the Royal Medical Society, and Resident Physician to the Clinical Wards of the Royal Infirmary, Edinburgh.

Few things are more distressing to a physician than to stand beside a suffering patient who is anxiously looking to him for that relief from pain which he feels himself utterly unable to afford. His sympathy for the sufferer, and the regret he feels for the impotence of his art, engrave the picture indelibly on his mind, and serve as a constant and urgent stimulus in his search after the causes of the pain, and the means by which it may be alleviated.

Perhaps there is no class of cases in which such occurrences as this takes place so frequently as in some kinds of cardiac disease, in which angina pectoris forms at once the most prominent and the most painful and distressing symptom. This painful affection is defined by Dr. Walshe as a paroxysmal neurosis, in which the heart is essentially concerned, and the cases included in this definition may be divided into two classes.

In the first and most typical there is severe pain in the precordial region, often shooting up the neck and down the arms, accompanied by dyspnœa and a most distressing sense of impending dissolution. The occurrence and departure of the attack are both equally sudden, and its duration is only a few minutes.

In the second class, which from its greater frequency is probably the more important, though the pain and dyspnœa may both be very great, the occurrence of the attack is sometimes gradual, and its departure generally so ; its duration is from a few minutes to an hour and a half or more, and the sense of impending dissolution is less marked or altogether absent.

Brandy, ether, chloroform, ammonia, and other stimulants have hitherto been chiefly relied upon for the relief of angina pectoris ; but the alleviation which they produce is but slight, and the duration of the attack is but little affected by them.

In now publishing a statement of the results which I have obtained in the treatment of angina pectoris by nitrite of amyl, I have to observe that the cases in which I employed this remarkable substance belonged rather more to the second than to the first of the classes above described.

Nitrite of amyl was discovered by Balard ; and further investigated by Guthrie, who noticed its property of causing flushing of the face, throbbing of the carotids, and acceleration of the heart's action, and proposed it as a resuscitative in drowning, suffocation, and protracted fainting.

Little attention, however, was paid to it for some years, till it was again taken up by Dr. B. W. Richardson, who found that it caused paralysis of the nerves from the periphery inwards, diminished the contractility of muscles, and caused dilatation of the capillaries, as seen in the web of the frog's foot.

Dr. Arthur Gamgee, in an unpublished series of experiments both with the sphygmograph and hæmadynamometer, has found that it greatly lessens the arterial tension both in animals and man ; and it was these experiments—some of which I was fortunate enough to witness—which led me to try it in angina pectoris.

During the past winter there has been in the clinical wards one case in which the anginal pain was very severe, lasted from an hour to an hour and a half, and recurred every night, generally between two and four a.m. ; besides several others in whom the affection, though present, was less frequent and less severe. Digitalis, aconite, and lobelia inflata were given in the intervals, without producing any benefit ; and brandy and other diffusible stimulants during the fit produced little or no relief. When chloroform was given so as to produce partial stupefaction, it relieved the pain for the time; but whenever the senses again became clear, the pain was as bad as before. Small bleedings of three or four ounces, whether by cupping or venesection, were, however, always beneficial; the pain being completely absent for one night after the operation, but generally returning on the second. As I believed the relief produced by the bleeding to be due to the diminution it occasioned in the arterial tension, it occurred to me that a substance which possesses the power of lessening it in such an eminent degree as nitrite of amyl would probably produce the same effect, and might be repeated as often as necessary without detriment to the patient's health. On application to my friend Dr. Gamgee, he kindly furnished me with a supply of pure nitrite which he himself had made; and on proceeding to try it in the wards, with the sanction of the visiting physician, Dr. J. Hughes Bennett, my hopes were completely fulfilled. On pouring from five to ten drops of the nitrite on a cloth and giving it to the patient to inhale, the physiological action took place in from thirty to sixty seconds; and simultaneously with the flushing of the face the pain completely disappeared, and generally did not return till its wonted time next night. Occasionally it began to return about five minutes after its first disappearance; but on giving a few drops more it again disappeared, and did not return. On a few occasions I have found that while the pain disappeared from every other part of the chest, it remained persistent at a spot about two inches to the inside of the right nipple, and the action of the remedy had to be kept up for several minutes before this completely subsided. In almost all the other cases in which I have given it, as well as in those in which it has been tried by my friends, the pain has at once completely disappeared. In cases of aneurism, where the pain was constant, inhalation of the nitrite gave no relief, but where it was spasmodic or subject to occasional exacerbations it either completely removed or greatly relieved it. It may be as well to note that in those cases in which it failed, small bleedings were likewise useless.

From observations during the attack, and from an examination of numerous sphygmographic tracings* taken while the patients were free from pain, while it was coming on, at its height, passing off under the influence of amyl, and again completely gone, I find that when the attack comes on gradually the pulse becomes smaller, and the arterial tension greater as the pain increases in severity. During the attack the breathing is quick, the pulse small and rapid, and the arterial tension high, owing, I believe, to contraction of the systemic capillaries. As the nitrite is inhaled the pulse becomes slower and fuller, the tension diminished, and the breathing less hurried. On those occasions when the pain returned after an interval of a few minutes, the pulse, though showing small tension, remained small in volume, and not till the volume as well as tension of the pulse became normal, did I feel sure that the pain would not return.

As patients who suffer from angina are apt to become plethoric, and greater relaxation of the vessels is then required before the tension is sufficiently lowered, I think it is advisable to take away a few ounces of blood every few weeks. When the remedy is used for a long time, the dose requires to be increased before the effect is produced. A less quantity is sufficient when it is used with a cone of blotting-paper, as recommended by Dr. Richardson, than when it is poured on a large cloth. From its power of paralysing both nerves and muscles, Dr. Richardson thinks it may prove useful in tetanus; and I believe that, by relaxing the spasm of the bronchial tubes, it might be very beneficial in spasmodic asthma. I have tried it in a case of epilepsy, but the duration of the fit seemed little affected by it. It produces relief in some kinds of headache, and in one of neuralgia of the scalp it relieved the severe shooting pain, though an aching feeling still remained.

While cholera was present in Edinburgh during last autumn, Dr. Gamgee proposed it as a remedy during the stage of collapse, a condition in which there are good grounds for supposing that the small arteries, both systemic and pulmonic, are in a state of great contraction. No well-marked case afterwards occurring in the town, he was deprived of an opportunity of putting it to the test, but it is a medicine well worthy of a trial, and should another epidemic unhappily occur it may prove our most valuable remedy.—*Lancet, July* 27, 1867, *p.* 97.

DISEASES OF THE ORGANS OF RESPIRATION.

23.—THE DIAGNOSIS AND TREATMENT OF DISEASES OF THE CHEST.

By Dr. Edward Charlton, Physician to the Newcastle-on-Tyne Infirmary.

[Our opinions as to the pathology of tubercle will have to be modified from the recent discovery that it is inoculable. Virchow considers that true tubercle is a disease *sui generis*, produced within the lungs by constitutional causes, and formed from elements already existing there.]

"It consists," says the Berlin pathologist, "of miliary masses of more or less transparent-cells containing one or many nuclei, so far resembling pus-cells as being developed like them from epithelium or connective tissue-cells, but differing from pus-cells in that they do not as a rule break up into a pathological cream, capable of being absorbed; but after being massed together in the form of miliary semitransparent tubercles, they break up by a retrograde metamorphosis, beginning in the centre, into a fatty granular detritus, becoming more fully softened by inflammatory exudation from the surrounding tissues."

Much as the medical world is indebted to Virchow for having pointed out the nature of true tubercle, it is perhaps still more important that he has shown the existence of a spurious tubercle, a true pus, which from some deficient vitality never breaks down into the ordinary cream, but may harden and contract into a low organic form, and remain as a permanent cheesy or cretaceous deposit. These deposits act as foreign bodies, they excite inflammatory action in surrounding parts, just like true tubercle; as grey, or particularly as cretaceous particles, they may be expectorated, but they are not the results of true phthisis. They may occur from local irritation, they may be caused by irritating particles inhaled into the lungs, as the silicious dust from dressing freestone; but the tubercular diathesis, the true constitutional disease, is not then present. It is in such cases that there are probably formed the cretaceous masses so frequent at the summit of the lungs, with the condensed pulmonary tissue, and the puckered up pleuritic covering, which has of late years been so triumphantly pointed to as evidence of the complete cure of consumption. There are few amongst us who will not have met with these deceptive cases, especially among stone-masons, and dry grinders of metal. We never received a stonemason into hospital with the general symptoms of phthisis, but we felt a hesitation and a doubt as to the true character of his case. When the expectoration contained cretaceous particles we always felt more hope than in ordinary phthisis, for we had seen so many of these cases do well. Some indeed ended fatally under our care; and in *post mortem* examinations we found that death had ensued from pneumonia, and not from true tubercular disease. This was pointed out to the profession twenty years ago, by Dr. Calvert Holland, but it was reserved for Virchow to explain the presence of these cretaceous masses, and to distinguish between true and false tubercle. Virchow has shown, too, that ordinary sarcoma, and even cancer, which we might regard as the very opposite of tubercle, may so degenerate as to be undistinguishable by mere inspection from the latter. The grey tubercle is then the true tubercle of phthisis; the yellow opaque tubercles are in many cases the result of ordinary inflammation and exudation. We will not, however, deny that inflammation may exist in, and may even be concerned in the production of true tubercle in a secondary sense; but under the influence of the tubercular diathesis this inflammation does not produce its ordinary results, a retrograde metamorphosis ensues, and the true tubercle may either dry up and wither into cartilaginous corpuscles incapable of further change, producing thereby a temporary cure, or it may go through the usual process of softening. This softening is of course rarely accomplished without the destruction of a considerable portion of the neighbouring pulmonary parenchyma, and thus, by the simultaneous or successive softening of masses of tubercle, certain portions of pulmonary tissue are isolated, loosened, and at length expectorated *en masse* from the lungs. Such portions are generally very small, and enveloped as they are in mucus they may escape detection. Recently it has been proposed by Dr. Fenwick, formerly of North Shields, to collect these fragments by boiling the mucus and pus of the expectoration in liquor potassæ, which liquefies the mucus and allows the heavier particles of lung-tissue to fall to the bottom of a conical glass, where they can easily be detected by the microscope. Now such evidence as this is really most valuable, for in the spurious form of phthisis no such destruction of lung-tissue takes place. Virchow has then taught us the existence of a true and of a false phthisis, he has proved that tubercle originates from abortive cell development, but he has not yet informed us whence originates the failure in the development of the normal cell. Is it in the brain from deficient nervous influence? is it in the stomach from imperfect digestion and assimilation? is the liver at fault? or, lastly, are we to seek the cause in deficient action or secretion of the pancreas? The latter is Dr. Dobell's theory, but we dare not yet pronounce it to be the true one. Here is a wide field laid open for investigation, a field in which there are already many ardent workers, but the further we advance in this direction the more arduous do our researches become.

It cannot perhaps be said that since the era of Laennec we have made great advances in the diagnosis of phthisis by auscultation and percussion. We have, however, greatly simplified the complicated nomenclature of sounds heard in the chest, we recognise the existence of moist and of dry sounds, but we no longer perplex the student with the minute difference between supercrepitant and subcrepitant *râles*, or between bronchial air-sounds or voice-sounds and pectoriloquy. We acknowledge in them a simultaneous cause differing only in degree. Another means of diagnosis has recently been added to our list, viz., the rise in the temperature of the body in phthisical disease as ascertained by the delicate clinical thermometers now in use. In fever, thermometric observations on the skin have been already found to be of great value, especially as regards the prognosis; but will the thermometer enable us to distinguish between true and spurious phthisis? We think not; for in both there is inflammatory action, which may and will elevate the temperature of the skin. After all, our predecessors, who knew nothing of auscultation or percussion, who were obliged to form their diagnosis upon the general history of the case, and the outward signs they could observe, were not wasting their time, and at the present day we reap the benefit of their observations; we readily acknowledge that without an accurate knowledge of the early details, without a carefully considered history of the symptoms, we may, in spite of our boasted skill with the stethoscope, fall into grevious mistakes.

While, then, we concede that spurious phthisis may be, and often is, perfectly cured, can the same be said of true tubercular disease? If Virchow's doctrine be correct, that the true grey semitransparent tubercle may do and occasionally does, at an early period of its existence, dry up, obsolesce, and cease to be dangerous, can we cure the diathesis, the faulty assimilation, the defective nervous influence, the failure or alteration of the pancreatic juice, which may be the cause of the production of tubercle? Can we decide on the existence of the tubercular diathesis before the perilous matter is formed in the lungs? With certainty we cannot, but we do, with most of the best pathologists of the present day, acknowledge the identity of the scrofulous and tubercular diathesis, and the former exhibits its presence by outward signs often appreciable at a very early period of life. The old physicians were right; the existence of scrofula in a family or in an individual should make us prepare against the invasion of tubercle, and adopt, as far as lies in our power, those hygienic measures which may ward off the disease. Have we, then, with our increased acquaintance with the nature of tubercle, advanced *pari passu* in the treatment of this dreaded disease? Is the mortality from phthisis lessened, or does it not still form one of the most important items in the bills of mortality? Do not the hurry and haste in which we now exist, the wild passion for money-making, the wilder and still more frivolous pursuits of fashion or pleasure, compensate for our better knowledge of tubercular disease? To a certain extent this may be the case; but from the time when Sir James Clark, thirty years ago, published his admirable essay on climate in the treatment of phthisis, a better prospect has been opened to us, and some here may live to see the day when the hygienic and the prophylactic treatment of phthisis will not be left merely to the isolated opinion of the medical attendant, but, like the great popular questions of vaccination, of cholera, and other epidemic diseases, will become a subject of careful legislational enactment. Still it cannot be said that the medical profession has been backward in the attempt to stay by treatment the ravages of phthisis. Every new remedy (and their name is legion) has been carefully tested; and meanwhile great and marvellous has been the change that within the last thirty years has occurred under our own eyes in the therapeutics of this malady. The old shutting up in a room and lowering treatment are exploded; tonics and open air exercise, whenever the latter can be borne, have taken the place of these. Iron and quinine have replaced the lancet and sedatives; it has been found that the cough is moderated while the appetite is improved by these tonics; and assimilation is far more vigorously carried on, than when the digestive powers were weakened by the constant repetition of opiates. Great attention is now paid to the functions of the skin; we have become altogether a bathing and washing people to an extent that our forefathers never dreamed of. That cod-liver oil has had its day and will soon be numbered among the things of the past, we are by no means willing to allow. We regard this remedy as really valuable, as an easily assimilated fat, helping to support the system while we strive by other tonics to alter the tubercular diathesis. It may be that the appearances of returning health that it produces are often fallacious, but we have ourselves, we are sure, seen many cases where its use has long protracted the disease. Of late years the statistics of phthisis have shown us that the malady is of much longer average duration than was laid down by Louis thirty years ago. We have indeed improved since then in the power of detecting the early stage of the malady; but, even allowing for this, we are disposed to believe that the greater average duration of phthisis at the present day is due partly to the tonic and more rational invigorating treatment employed, and partly to the free use of cod-liver oil. But there is another circumstance which above all has contributed powerfully to prolong the phthisical patient's existence, and in some few cases perhaps to effect a cure. The facilities afforded by railway travelling are now so great that in a few hours the ailing invalid can leave our Arctic winter, and our still more bitter spring, and be enjoying the soft southern breezes and open air exercise on the northern or southern shores of the Mediterranean. The exodus of the healthy and their families from England during the winter and the spring months is as yet only beginning. In a few years, if peace continue to reign on the continent of Europe, it will be infinitely more extensive; indeed we anticipate that no one who can afford a winter's residence abroad will care to stay at home. While the phthisical patient of limited means must, during our winter and spring, remain the greater part of the time within doors, the sojourner at Mentone, or at Hyères, may be out for hours each day in the open air. During the last winter, a season of such severity as will be remembered by us all, the thermometer at Hyères only sank one single night to the freezing point. To how many scrofulous and hence possibly phthisical children may not this southern annual journey have been the means of stopping the first advances of tubercular disease, and in how many cases of the already established malady may not valuable lives have been prolonged for months and perhaps for years.—*British Medical Journal, Aug.* 24, 1867, *p.* 146.

24.—ON THE NATURE AND THE MODE OF PROPAGATION OF PHTHISIS.

By Dr. William Budd, Consulting Physician to the Bristol Royal Infirmary.

The following are the principal conclusions to which I have been led regarding Phthisis or Tubercle :—

1st. That tubercle is a true zymotic disease, of specific nature, in the same sense as typhoid fever, scarlet fever, typhus, syphilis, &c., &c., are.

2nd. That, like these diseases, tubercle never originates spontaneously, but is perpetuated solely by the law of continuous succession.

3rd. That the tuberculous matter itself is (or includes) the specific morbific matter of the disease, and constitutes the material by which phthisis is propagated from one person to another, and disseminated through society.

4th. That the deposits of this matter are, therefore, of the nature of an eruption, and bear the same relation to the disease, phthisis, as the "yellow matter" of typhoid fever, for instance, bears to typhoid fever.

5th. That by the destruction of this matter on its issue from the body, by means of proper chemicals or otherwise—seconded by good sanitary conditions—there is reason to hope that we may, eventually, and possibly at no very distant time, rid ourselves entirely of this fatal scourge.

The evidence on which these conclusions are founded is drawn from the following principal sources :—

(*a*) Considerations based on the pathology of phthsis, as showing it to consist in the evolution and multiplication within the organism of a specific morbid matter, with a universal tendency to elimination and casting forth of the same, after the type of zymotic diseases generally.

(*b*) Actual instances in which there was evidence to show that phthisis was communicated from one person to another.

(*c*) The geographical distribution of phthisis in past and present times, and, especially, its great fatality now in countries which when first discovered by Europeans were known to be entirely free from it.

(*d*) Its much greater prevalence in low levels and among crowded communities, and its entire absence, unless by casual importation, at very high levels,—conditions which are well known to rule, in the same directions, the spread of zymotic diseases generally, and especially of that group in which, as in phthisis, the morbific matter is cast off in a liquid form.

(*e*) Its very high rate of prevalence in convents, harems, barracks, penitentiaries, &c.—that is to say, under the very social conditions which are known most to favour the propagation of diseases of the zymotic group.

Among the data relating to geographical distribution the following striking facts may be here mentioned :—

1st. When the South Sea Islands were first discovered phthisis did not exist there. Since the aborigines have come into intimate contact with Europeans the disease has not only made its appearance among them, but has become so widespread as to threaten their extermination.

The contrast between original entire immunity and present extreme fatality is very striking, and can only be rationally explained by the importation of a new and specific morbific germ.

Try every other supposition, and the facts are inexplicable ; make this one supposition, and they are at once explained.

2nd. The late Dr. Rush, of Philadelphia, who made very accurate inquiries to determine the point, satisfied himself that when America was first discovered, phthisis was unknown among the native American Indians. Now it is very fatal to them.

The very significant contrast here exhibited between the past and present history of these two races, in respect of phthisis, is exhibited at once, and at the present time, among the negro race in Africa, in different parts of the area of that great continent.

It is well known that negroes are peculiarly liable to phthisis.

Now, everywhere along the African seaboard where the blacks have come into constant and intimate relation with the whites, phthsis causes a large mortality among them. In the interior, where intercourse with the whites had been limited to casual contact with a few great travellers or other adventurous visitors, there is reason to believe that phthisis does not exist. Dr.

Livingstone and other African travellers have given me the most positive assurances on this point.

The idea that phthisis is a self-propagated zymotic disease, and that all the leading phenomena of its distribution may be explained by supposing that it is disseminated through society by specific germs contained in the tuberculous matter cast off by persons already suffering from the disease, first came into my mind, unbidden, so to speak, while I was walking on the Observatory hill at Clifton, in the second week of August, 1856. The close analogy in many quite fundamental points between this desease and typhoid fever had often impressed itself on me with very great force while I was engaged in the study of the latter, and in the preparation of the papers which I have published on it. I now saw with a clearness which had never occurred to me before, that, with the exception of the qualifications necessary for their application to a chronic disease—for the most part of slow evolution and indefinite duration,—the leading conclusions to which I had been led respecting the propagation of the fever, might be applied with the same strictness to phthisis also.—*Lancet, Oct.* 12, 1867, *p.* 452.

25.—CASE OF ACUTE TUBERCULOSIS.

Under the care of Dr. JENNER, at University College Hospital.

[The following short and interesting case is taken from the reports of practice at the London Hospitals in the Medical Times and Gazette. One often hears of cases of so-called acute tuberculosis, but the term is most frequently applied to instances of "galloping consumption," when patients die with all the appearances of advanced phthisis after six or eight weeks' illness; but the case related here is of a much more uncommon sort.]

A servant girl, aged 25, was on June 6th rather out of sorts, but nothing particularly wrong was noticed; even on the 9th she only complained of headache, but on the 10th she felt so ill that she was obliged to keep her bed. On the 14th she was brought to the hospital. When admitted, she complained of little beyond a slight frontal headache; yet it was evident that something unusual ailed her, from her pale face, dull eyes, dry, brown, and tremulous tongue, and the sordes on the lips and teeth. But what was it? The condition of the chest was normal, but the abdomen seemed rather full and tender on deep pressure. There were no spots on the surface, whilst the pulse was full, regular, and only beating 72 a minute. The diagnosis lay between tuberculosis and typhoid fever. By evening all aid to be derived from the patient's sensations was removed, for she became totally insensible. The thermometer, also, which might have given some indication, was at fault, the temperature being 103·2°—the temperature of fever rather than of tuberculosis. On the evening of the 15th her pulse was quicker—120—regular, and of fair strength, but otherwise she was extremely prostrate, and died on the morning of the 16th, exactly ten days from the time she first felt out of health, and six from the time she took to bed. The mystery which surrounded the case in life was removed by a post-mortem examination, which showed the whole peritoneum thickly studded with tubercles, and the omentum thickened by a deposit of the same kind. The mesenteric glands were enlarged, and the peritoneum round them somewhat vascular; but there was only a small quantity of fluid in the cavity of the abdomen. There were tubercles in the stomach, tubercles in the kidneys, tubercles in the pleura, and many tubercles in the lungs. Tubercles there were also in the pia mater of the brain, about the fissure of Sylvius, and the cerebral ventricles contained an unusual quantity of fluid, whilst the brain matter around was somewhat softened. In fact, it was apparent that the whole body was tubercular, and that the patient had died of acute tuberculosis.—*Medical Times and Gazette, June* 27, 1867, *p.* 90.

26.—ON THE INOCULATION OF ANIMALS AS A MEANS OF DIAGNOSIS IN TUBERCULAR PHTHISIS.

By Dr. William Marcet.

We are indebted to Dr. Villemin for the very remarkable discovery that tubercular phthisis can be inoculated from man to animals; rabbits and guinea-pigs, which are naturally liable to the disease, being fit subjects for inoculation. When I became acquainted with Villemin's papers, I happened to be engaged with an investigation of the expectorations in phthisis, and it struck me that, if his results were correct, by inoculating the expectorations of phthisical patients, containing tubercular matter, to healthy rabbits or guinea-pigs, these animals would become tubercular; thus I thought that a physiological method of diagnosis of great importance might be arrived at, for should the inoculated animals, after a certain time had elapsed, either die tubercular, or, on being killed, exhibit tubercles in any part of their body, there could be no more direct evidence as to the nature of the diseased condition of the patient whose expectorations had been used for inoculation. If the result of the inquiry was found to be in accordance with this view, I had yet to show that the non-appearance of tubercles in guinea-pigs inoculated with expectorations from a supposed incipient case of phthisis might be considered as evidence of the patient's lungs not being tubercular. This second part of the question is more difficult to solve than the first; I am engaged with it at present. Twenty-two guinea-pigs were submitted to experiment. The expectorations of nine different patients suffering from phthisis were inoculated to as many guinea-pigs; in two cases two animals were inoculated respectively with the sputa from the same patient. Of these 9 patients, 8 could be safely considered in the second stage of phthisis, the tubercles undergoing softening and being expectorated. One patient appeared to be in the chronic third stage, with cavities contracting. The sputa of one well-diagnosed case of bronchitis was inoculated to a guinea-pig, for the sake of comparison, and one young animal of the same litter as two which were inoculated was kept without being inoculated for a similar purpose. This will account for thirteen of the animals. The remaining nine were used in these experiments as follows:—Two guinea-pigs were inoculated with the expectorations of two cases of doubtful phthisis. One animal was inoculated with blood taken from a body during a post-mortem examination at the Brompton Hospital; another with blood procured from the last animal eleven days after the former operation; two were inoculated with pus taken from the chest in a case of empyema, where paracentesis thoracis had been performed; lastly, three sound guinea-pigs, kept with those that had been operated on, were killed in order to ascertain whether their internal organs were free from tubercles. The following results were obtained from the inoculation of the eleven guinea-pigs with the expectorations from phthisical patients;—One of the animals died three days after the inoculation, obviously from some cause independent of the influence which the inoculated matter might have exerted towards the development of tubercles, and in this case no tubercle could be found in the body of the animal. Six guinea-pigs died, and every one of them exhibited most positively tubercles to a greater or less extent. Of these five died between forty-three and fifty-six days after being inoculated, and one case proved fatal in twenty-one days, on February 7, the cold weather having obviously weakened the animal, and contributed to the rapid termination of the disease; in the present case the guinea-pig's spleen alone was found tubercular. Four other guinea-pigs were killed at periods varying from forty-seven to fifty-four days after inoculation, and tubercles were found in every one of these animals. There are seven more cases of inoculation to account for—1st. Inoculation with expectorations from a patient exhibiting doubtful signs of tubercles. Results: Death two days after inoculation from the effects of the operation; no tubercles found. 2nd. Inoculation from another doubtful case of phthisis; animal killed fifty days after the operation; no tubercles found. 3rd. Inoculation with blood from tubercular human dead body; animal killed eighty-three days after inoculation; tubercles found in lungs, liver, spleen, lymphatic and mesenteric glands. 4th. Inoculation with blood from last animal eleven days after the operation; guinea-pig killed seventy-two days later; no tubercles. The stage of the disease at which the blood was taken may have been too early to be productive of tubercles; hence the negative result. 5th and 6th. Inoculation of the two guinea-pigs from the case of empyema; result, one animal died eight days, and the other was killed fifty days, after inoculation. The first guinea-pig dying so soon appears to show that it was tubercular when operated, which does away with the value of the experiment. The second exhibited tubercles in the lungs, liver, spleen, lymphatic and mesenteric glands. 7th. Inoculation with the expectorations from the case of bronchitis. The animal was found, on being killed eighty-two days after inoculation, in no way tubercular. Finally, not one of the three animals left sound, and which had lived with those that had been inoculated, presented, on being killed and examined, the slightest trace of tubercles, although they had been for about two months under precisely the same conditions as the others; and the young guinea-pig of the same litter as two of the inoculated ones, and which was preserved healthy, is now growing rapidly, and appears in perfect health. I may add that Mr. M. J. Salter, who assisted me in these experiments, has witnessed every one of the results I have recorded in the present communication. In conclusion, I believe the results of the experiments and observations related in the present paper to be as follows:—1st. The inoculation of guinea-pigs with the expectorations of patients suffering from phthisis will, at all events in a certain stage of the disease, and possibly throughout, give rise to the formation of tubercles in the operated animals. 2nd. If two or more guinea-pigs inoculated with human expectorations, brought up by coughing, should die from tubercular disease, or should, on being killed at least thirty days after inoculation, exhibit tubercles, this may be considered as a direct and positive evidence that the person whose expectorations were inoculated was suffering at the time from tubercular phthisis. 3rd. If two or more guinea-pigs be inoculated with the expectorations coughed up by a person considered to be in the third stage of phthisis, and if these animals do not die of tubercular disease, or exhibit any tubercles when killed at least fifty days after inoculation, it may be considered that in the present case the softening of tubercles and the secretion from the pulmonary cavities are arrested, the patient being in a fair way of recovery. 4th. Other materials besides the pulmonary expectorations taken from the human body in certain, if not in all, stages of phthisis, as blood and pus, appear to be also possessed of the power of causing the formation of tubercles in guinea-pigs when inoculated to these animals. 5th. The spleen appears to be the first, and the lungs one of the last, organs in guinea-pigs to be attacked with tubercular disease.—*Medical Times and Gazette, Sep.* 28, 1867, *p.* 361.

27.—ON THE INFLUENCE OF ALPINE CLIMATES ON PULMONARY CONSUMPTION.

By Dr. Hermann Weber, Physician to the German Hospital.

Case 1.—H. F., aged 31, a German, came to London in 1852, had a chronic cough in 1853, and hemoptysis in the summers of 1854 and 1856. He spent then seven months in Cairo, where he became better, without, however, entirely losing his cough. He had again hemoptysis in March 1857 in Cairo, and much cough during the summer of 1857 in London. There was improvement during the following winter at Cannes and Nice; but he was much worse after his return to London. He was first seen by myself in July 1858. He had lost 29 lb. in three years. There was dulness on the left side from the apex to the fifth rib, with bronchophony and crackling rhonchus in the supra- and infra-clavicular spaces; and an analogous condition on the back of the same side. Nothing abnormal was discovered on the right side. He had shortness of breath. Pulse 85; thoracic expansion, 35 to 36½ inches. He went to Valparaiso, and had there again hemoptysis. He spent then eight months on the Peruvian Andes (at an elevation of between 9,000 and 10,000 feet), and gained flesh and lost cough. He went afterwards to New Orleans, where he again began to cough and lose flesh. He was sent thence to the table-land of Mexico, where, to his own feeling, he entirely recovered his health. When seen by me in the autumn of 1860, the dulness on the left side reached only from the apex to the third rib. This portion was much flattened; and there was absence of vesicular breathing, but no bronchophony, and no rhonchus. He had gained 24 lb. in twenty-seven months. Pulse 66; expansion of chest, 36 to 38 inches. He remained well until the spring of 1866, when he again began to cough, was often feverish, lost much flesh, and had some hemoptysis. When seen in the beginning of October 1866, the old affection of the left side had remained almost unchanged; but on the right there was dulness from the apex to the fourth rib, with bronchophony and occasional crackling rhonchus in the subclavicular space. He had lost 20 ℔. in twelve months. Pulse 95; chest-expansion, 35½ to 37 inches. He went to the Valley of Jauja, in the Peruvian Andes, and lost his cough almost entirely; and when seen in June 1867, he had gained 15 ℔. The dulness descended on the right side only to the second rib. There was no rhonchus, but scarcely any respiratory murmur over the dull space. Pulse 74; expansion of chest, 36 to 38 inches.

The rapid improvement repeatedly obtained in this case by the removal to elevated regions is very striking; and it is also important to remark, that the improvement effected by the stay at Cairo and in the South of France was neither so great nor so lasting as that from the mountain sojourn.

Case 2.—B. D., from the lower parts of Switzerland, came to London in 1864, aged 22. He was much depressed by the fogs and dulness of the atmosphere. He had repeated attacks of bronchitis during the winter; and in the spring of 1865 he coughed and became emaciated during the summer. There was slight dulness in the left clavicular and infraclavicular region with diminished respiratory sounds. He spent the winter at Bordeaux and at Cannes; he never lost his cough entirely, and continued to lose weight. He returned to London in May 1866. Dulness extended on the left side from the apex to the third rib. He had, early in June, an attack of sore-throat, which was with him the usual forerunner of bronchitis; he became feverish, and at the end of the month a pleuropneumonic affection was ascertained in the lower part of the right side, which gradually crept upwards to the upper third of the scapula. By the end of July, the pyrexia had almost ceased, under a treatment of milk, quinine, and cod-liver oil; but the dulness over the back of the right side had not yielded. There was crepitant rhonchus over the upper part of the dull space, and absence of respiratory murmur over the lower; pulse 92 to 100; expansion of chest, 31¾ to 33 inches. The dyspnœa was great at every exertion; frequent cough, with about two and a half ounces of mucopurulent expectoration. He had lost 27 ℔. in as many months' stay in London. In this state he went, towards the end of July, to the Righi (Kaltbad and Scheideck), lived there almost entirely in the open air, and drank about two quarts of milk every day. In less than six weeks he was able to take considerable walks; and in October he had lost his cough almost entirely. He spent the winter in different parts of Switzerland, usually above 2,500 feet above the sea-level. He had not a single attack of sore-throat and bronchitis; and when he returned to London, towards the end of May, the right side was quite normal; the left side was in front, the supra-

and infra-clavicular spaces, slightly sunk in ; there was moderate dulness and scarcely perceptible breathing from the apex to the second rib. He gained 19 ℔. in weight during ten months, and felt in every respect very well.

Case 3.—B. K. aged 21, had pneumonia of the left side about ten years ago. He came to London in spring 1865, and began to cough and lose flesh in the winter 1865-66. Hoarseness supervened in spring 1866, and the cough gradually increased. When seen in July, he was thin and pale, with circumscribed red cheeks ; he had chronic swelling of the tarsal portion of the eyelids ; pulse 105 to 110 ; respirations 26 to 30 ; temperature every evening slightly increased. There was fresh pneumonic affection of the lower part of the left side ; dulness and absence of respiratory murmur on the right from the apex to the third rib. The evening pyrexia gradually subsided under perfect rest and milk diet ; but the pneumonic affection slowly crept upwards, the dulness extending on the posterior part of the left side from the bottom to the upper third. When he left England towards the end of August, there was crepitation with slight bronchophony over the greater part of the left scapula ; and the older affection of the right side had remained unchanged. He was advised to go to the Davos, but went first to Germany ; and only after having been attacked by fresh colds he repaired in November to Davos am Platz, where he stayed until the middle of April, and, in spite of a rather unfavourable winter, entirely lost his cough, and gained 7 lbs. in weight ; and the spirometer showed an increase of pulmonary capacity amounting to more than 900 cubic *centimètres.* According to Dr. Unger's report at the time of A. B.'s departure from the Davos, nothing abnormal could be discovered on the left side, except, perhaps, slight dulness in the lowest part ; and the dull space on the upper portion of the right side was likewise diminished. In addition, the affection of the eyelids, which had existed for several years, had entirely disappeared without remedial interference.

With regard to the experience collected at Davos am Platz, accurate records date only from February 1865, when Dr. Unger, himself formerly consumptive, and then not yet quite recovered, came to the village, accompanied by a young gentleman with extensive lung-disease. Between February 1865 and March 1867, 35 patients with consumptive affections had been under the observation and treatment of Drs. Spengler and Unger, who have kindly furnished me with a report ; of which, however, I can only give a very short summary. Of these 35 cases, only 1, belonging to a consumptive family, offered no distinct signs to auscultation and percussion, although the spirometer showed a considerable impairment of the respiratory capacity. Of the other 34 cases, 19 were in the first stage, with distinct deposits; 15 in the second, with cavities. Of the 19 in the first stage, 2 had deposits in both sides and some had other complications, as pleuritic effusion; 7 of the 19 left cured, 6 improved, 6 remained under treatment; 18 of the 19 had gained in weight; 8 had previously had attacks of hemoptysis, which only in one of them returned in a very slight degree during the stay. Attacks of fresh colds were remarkably rare in all of them. Of the 15 in the second stage, only 2 were free from fever; 3 of them had pleuritic exudations; 5 of the 15 died (3 from gradual consumption, 2 from hemoptysis); 1 left worse than he arrived; 1 improved; 2, who had arrived with considerable pulmonary distension, are steadily improving; and 1 is regarded as perfectly convalescent. It is worthy of remark, that diarrhœa did not occur in any of them, and that the night-perspiration in almost all cases disappeared after a short stay. In one case only, careless exposure led to a croupous pneumonia of the left lower lobe, with a favourable termination on the fifth day.

The treatment adopted at the Davos consists in the use of much milk, and light nourishing food; a moderate amount of wine, principally the red wine of the Valteline; and graduated exercise, first on level, later up hill. The cold douche is likewise in many cases used with advantage.

Those who consider the facts related will probably not deny that they offer testimony in favour of mountainous climates. The two cases which at a later period ended fatally are particularly instructive, by showing that a curative process had taken place during the stay in elevated regions. Both cases, and probably also the three others related, belonged to the class described by Addison under the head of pneumonic phthisis. It is probably on these low forms of catarrhal pneumonia, with a tendency to chronic infiltration, cheesy transformation, and formation of cavities, that the Alpine climates have a truly curative and prophylactic influence.

The question, how the mountain climates exercise so favourable an influence, can probably not be answered without entering the field of theories, which I wish to avoid as much as possible. There are, however, some undeniable facts. Thus the air of elevated regions is lighter, more rarefied, and cooler; and it is usually free from the foreign admixtures found in towns, and also free from the various kinds of malaria. The food, the occupation, and the whole manner of living, are generally different from those prevailing in large towns and much inhabited districts. The influence observed by most people on their removal to mountainous climates is an improvement of appetite, digestion, formation of blood, and nutrition in general. Exhilaration and increased inclination to exercise are likewise frequent attendants of the change All the influences enumerated to which others might be added, are of importance in the consideration of the question before us. I will only allude to one; viz., the rarefied air. A given volume of air contains absolutely less oxygen and other constituents at an elevation of 5,000 feet, than at the level of the sea. The movements of the thorax must, therefore, be increased, if the same amount of oxygen is to be introduced; or a smaller amount of oxygen will be introduced if the movements of the thorax remain the same. It has been asserted, but, as far as I know, not yet been proved experimentally, that the inspirations on mountains are deeper. Healthy persons at all events, usually have an inclination to take deeper inspirations; and the broad chest of mountaineers may be regarded as an effect of increased respiratory movements. If this view were correct, it might be asked, how far this augmented action of the organs of respiration, and, as a necessary consequence, those of circulation, influences the sanguification and all the processes of nutrition—how far, I say, this mere increase of chest-expansion would go to explain the facts before us. All influences, climatic, physical, or mental, by which the breathing is habitually increased, appear to act beneficially in the tendency to consumption; while all those causing diminished breathing seem to favour, in man and animals, the development of scrofulous and catarrhal inflammations, and of consumptive diseases of the lungs in general.

Although I have already occupied too much space, I must still add a word of caution. Not all consumptive constitutions probably are fit for the Alpine climate, and not all the stages of consumptive disease may be benefited. The invalid ought not to be allowed to go to any Alpine climate he likes, and do there as he pleases; but he ought to be directed to a place where he can have the advice of a medical man who will carefully superintend his diet, his exercise, and his whole manner of living. Many invalids lose the benefit obtained in the commencement by over-exercise and careless exposure; and the very help which these climates have given them may be turned by them to their ruin. It is further necessary that the invalid be sent in an early stage of the disease, and that he remain long enough, if possible, to regain the full capacity of healthy lungs; and that he be not allowed to return too early to his former unhealthy residence and occupation. On all these points, however, I cannot suggest better advice than that embodied in the excellent works of Sir James Clark on the *Sanative Influence of Climate* and on *Pulmonary Consumption.—British Medical Journal, Aug.* 24, 1867, *p.* 148.

28.—ON THE ACTION AND USES OF PHOSPHATE OF SODA IN SMALL DOSES.

By Dr. Wm. Stephenson, Extra-Physician to the Royal Hospital for Sick Children.

[Phosphate of soda is usually described as a mild saline aperient. Its action is such, however, only when given in large doses, and in the present day it is but rarely prescribed.]

It is an important salt in the animal economy, and one often sadly deficient in the food of children. A further reason for its use he bases on the experiments of Dr. Marcet, who first pointed out that the fatty acids are converted into an emulsion by its agency, and that they thus may become more easily assimilated. This is certainly a point deserving of more attention at the hands of medical men than it has received. Further than this, I have not found any special therapeutic action ascribed to it when administered in small doses. That it has such, however, it is the object of this paper to show.

My attention was first drawn to it by seeing it frequently prescribed by my colleague, Dr. Ritchie, who uses it according to the recommendations of Dr. Routh. The first case I gave it in was for a child, four months old, who was being artificially reared on the bottle, and although healthy in every other respect, was somewhat confined in the bowels, having but one motion each day, copious, but dry and white, exactly like coarse white chalk and curdled milk. A pinch of the phosphate of soda was ordered to be added to each bottle of milk. The result was striking, for the very next day the motions were in every way natural and healthy. I had afterwards several occasions of observing in this child the same action under similar circumstances.

The extensive field for observation afforded me at the Children's Hospital has enabled me further to investigate the matter; and in order to have as few disturbing influences as possible, the remedy was always prescribed alone, the parents being directed to put a pinch of the powder into each article of food the child received: in this way four or five grains were administered each time. The class of cases for which it was ordered were wherever, from an unhealthy character of the motions, the visceral secretions seemed to be abnormal. The first point that became apparent was, that in a great majority of cases where the colour of the stools was abnormal the natural yellow appearance was restored. This was the only appreciable result in many cases; but, at the same time, it was observed that some forms of diarrhœa at once yielded to its action. Several cases of chalky motions, similar to the one narrated, have been treated with the same beneficial results; and now, in this disorder, I have entirely discarded gray powder, and rely on the phosphate alone. Thus far an action of the liver is clearly indicated, which becomes confirmed by the result in the following cases of jaundice :—

Case 1.—Wm. P., a robust child, 4 years of age, was brought to the hospital suffering from jaundice. The skin and conjunctiva were of a deep yellow colour. Four months before, he had an attack of scarlatina, and since then he has often complained of pain in the belly, his bowels being more or less deranged, motions sometimes white, never a natural colour. Six days ago he turned sick, and vomited for two days ; no purging ; great thirst. No bilious-looking matter in the vomit. On the second day of the attack his skin was observed to be yellow, his stools white, and urine high coloured, staining whatever it touched. Ordered 30 gr. phosp. sod. in his food each day. The next morning his motion was slightly yellow, and water much clearer. On presenting himself the following day, a marked improvement in his colour was perceptible, and in a few days he had quite recovered.

Case 2.—The notes of the following case were supplied by Dr. Ramsay, my assistant, who attended the child :—

On the 1st of February, I was called to visit a little boy of 2 years of age, whom I found to be suffering from well-pronounced jaundice. The mother believed the jaundice to have been caused by the child's "taking a draw" from his father's tobacco pipe. His skin and conjunctiva were deeply tinged with bile. The urine was of dark porter colour, and the stools were pale and clay-coloured. He was languid and dull, but complained of no local uneasiness. I ordered him to be fed upon bread and milk for a day or two, and to have as much of the phospate of soda as lay on a threepenny-bit night and morning. At next visit, *i.e.*, on the following day, I found that, having lost the prescription, my patient had had none of the phosphate. He was still more deeply jaundiced than when I first saw him, and the lassitude had increased. Again ordering the phosphate, I left, and did not return till the next day but one. There was marked improvement. The skin and conjunctiva were nearly normal in appearance, the urine was of a natural colour, and the stools had regained the appearance of healthy fæces. I only saw him once subsequently, when I found him in perfect health.

Jaundice in children, however, may pass away of itself ; too much weight, therefore, must not be placed on such cases. The improvement in these two instances was certainly immediate upon the administration of the medicine, and may accordingly be taken into consideration along with other cases of a different character where the liver is involved.

In children, we find diarrhœa associated with inaction of the liver, as indicated by the uniform white colour of the motions. The following cases illustrate this condition :—

Case 3.—A boy, 3 years and 4 months old ; can neither walk nor speak, and was 14 months before he cut his first tooth. He has been very subject to diarrhœa, but looks well, and is not emaciated, although soft in the flesh. 19th January. Has had diarrhœa for six weeks ; motions uncountably frequent, quite watery, pale white, never yellow, and having a strong smell. Portions of egg have been observed to pass through the bowels ; vomits occasionally ; has a very poor appetite and great thirst. Ordered phosphate of soda.

24th. Bowels have been moved only three times in the twenty-four hours ; discharges still watery in the morning, but stiff through the day, slightly yellow, and more natural in appearance. He is taking his food better, and has not so much thirst.

2nd February. Bowels still rather loose in the morning, otherwise natural ; he has no thirst ; appetite good, and he is rapidly gaining strength.

Case 4.—A girl, 18 months old ; weaned at 7 months. 10th April. Has had diarrhœa for two months ; six or eight motions each day for the last two weeks ; they have been uniformly white in colour and very watery ; vomits occasionally. Ordered phosphate of soda.

12th. An hour after taking the powder, the motions became yellow ; they are still watery, but only twice in the day. Vomiting continues. To continue the powder, and give pepsine wine after each meal.

17th. Vomiting stopped ; bowels again loose and stools white. The phosphate of soda had not been stopped, but the quantity lessened. Ordered to increase the dose.

19th. Motions are again healthy in colour, and now of natural consistence.—*Edinburgh Medical Journal, Oct.* 1867, *p.* 337.

29.—ON THE INHALATION OF THE VAPOUR OF CHLORIDE AMMONIA IN ITS NASCENT STATE.

By Dr. Hermann Beigel, Physician to the Metropolitan Free Hospital.

[Ammonia is a remedy of great value in all catarrhal diseases of mucous membranes in general, and of those of the respiratory tract especially. Galen used to use the remedy locally but in a very primitive way. He directed his patients to burn pieces of hartshorn and inhale the vapour proceeding therefrom.]

It was Alfred Smee who not only gave distinct indications for the local application of ammonia by pointing out the diseases in which he had applied it with benefit, but who likewise constructed an apparatus for its convenient application. Mr. Smee's paper and woodcut of the apparatus were published in the London Medical Gazette of 1843 (vol. ii., p. 59). "The inhalation of so stimulating a gas as ammonia," says Smee, "is well known to be at first sight perfectly startling to those who have never either tried it on their own persons, or never seen it applied by others; but it is really in many cases, with proper management, a simple and one of the most delightful remedies that can be employed." The diseases in which Smee principally used the local application of ammonia were, chronic hoarseness, and old-standing cases of asthma; "especially in those in which the medical man considers that the internal use of the sesquicarbonate of ammonia is indispensable, in which the extremities are cold, the pulse feeble, and the general vital powers depressed, the local application of ammonia is particularly grateful; the patients feeling, as they describe it, a glow after its exhibition, and the warmth first imparted to the lungs extending by degrees over the whole system." Smee likewise recommends the gas as affording great relief and comfort in a relaxed, swollen, and apparently semi-œdematous state of the mucous membrane, which supervenes from remaining in crowded, overheated, and ill-ventilated rooms, where every person not only inhales his own breath over and over again, but is under the necessity of breathing his neighbour's also.

Powerful and beneficial as ammonia is as a remedy, yet it is connected with many inconveniences when applied as vapour spontaneously exhaled from solutions of ammonia. In this state, even if sufficiently diluted, violent symptoms follow each application; and, the gas being rapidly absorbed by the mucous membranes of the respiratory apparatus, the local effect cannot easily be controlled.

A far better plan of applying ammonia to the organs of respiration, therefore, is the one of Dr. Lewin, consisting in making use of the nascent state of chloride of ammonia, in which state it forms a vapour, and can readily be inhaled. But this vapour consists, in fact, of the atoms of a solid body, which, when inhaled, penetrate into the minutest vesicles of the lungs, and, becoming precipitated on the mucous membranes of the organs of respiration, afterwards get absorbed, thus exercising both a local and general effect.

This affords many important advantages. But before enumerating them, it will be necessary to explain the construction of the apparatus which I employ for the above purpose. It consists mainly of three bottles, as used in chemistry for the evolution of gases: one, containing some liquor ammonia, and another, containing some hydrochloric acid—according to their chemical proportions,—are connected with a third, which is about half filled with pure water. From the last bottle the inhaling tube projects. The whole apparatus is put into a box for portability.

The patient raises the mouthpiece of the tube to his mouth and uses it exactly in the same manner as a pipe is used by a smoker. With each draught, ammonia, escaping from the first bottle, and hydrochloric-acid gas from the second, combine, the moment they meet in the third, to form chloride of ammonia, which gets washed in the water of the last, and deprived of the superfluous particles of the two constituents.

The apparatus, which I have had made by Messrs. Krohne and Sesemann, of Whitechapel-road, is extremely simple, and requires no other attention than to renew from time to time the fluids. The advantages of this mode of local application of ammonia are various.

1. The vapour of chloride of ammonia thus smoked *in statu nascente*, may be swallowed or inhaled without any inconvenience: in the former instance becoming absorbed by the stomach; in the latter, being in contact with the mucous membranes of the mouth, pharynx, larynx, trachea, bronchial tubes, and, according to the force of inspiration, it penetrates into the minutest vesicles of the lungs.

2. The vapour being precipitated only after a few seconds' stay in the respiratory organs, it is in our power either to exhale it again after acting for a short time on the mucous membrane, or, by keeping the breath for a few seconds, to allow the vapour to precipitate on the mucous membranes.

3. A deep inspiration being required to drive the vapour forth to the finest air-tubes and lung-vesicles, it is to a certain degree in our power to act only on the pharynx and larynx, or to extend the action to the bronchial tubes and lungs.

4. By altering the proportions of the hydrochloric acid and the ammonia, the latter may be allowed to exist in a free state, and to exercise irritating effects. Should it, on the other hand, be required to deprive the vapour of all superfluous particles of ammonia, it would only be requisite to add some hydrochloric acid to the water in the third bottle; thus establishing two sources for originating chloride of ammonia.

5. If balsamic mixtures or volatiles be added to the water—for instance, essence of turpentine, camphor, &c.,—the efficacy of the smoke may be increased in appropriate cases.

6. The smoking and inhaling of chloride of ammonia is not in the least disagreeable.

It is true that a certain amount of practice is required in order to inhale the smoke well; but this practice is acquired very soon indeed. The effect of these inhalations, in many cases, shows itself very rapidly; and I have seen instances of bronchial catarrh in which relief was felt after inhaling a few times only. Hoarseness, proceeding from a catarrhal state of the vocal cords, has likewise been relieved in some instances in which local application of astringent solutions have been without any effect. Difficult expectoration becomes often soon easy; and the vital capacity increases if it has been diminished by an undue action of the mucous membranes of the organs of respiration.—*Lancet, Oct.* 26, 1867, *p.* 512.

30.—ON INHALATION OF ATOMISED FLUIDS.

By Dr. **Hermann Beigel**, Assistant-Physician to the Metropolitan Free Hospital.

[Dr. Beigel in Chapter ii., Part I, of the work from which the following extract is taken, after discussing the question as to whether the atomised fluids really enters the minute passages, observes :]

"*It is therefore confirmed beyond doubt that atomised fluids enter the respiratory tract and penetrate into the very cells of the lungs; that, therefore, by means of inhalation, remedies most appropriately and successfully may be applied to the organs of respiration.*"

[In Chapter v. he observes :—]

The medicaments which can be atomised and which have been in that condition found to be beneficial in diseases of the respiratory organs are, according to Dr. Beigel :—*Nitrate of silver*, dose three to five grains in an ounce of distilled water, serviceable in inflammatory conditions of the pharynx and larynx; *nitrate of aluminium*, two to five grains in an ounce of water, first used by Dr. Beigel for inflammation, and also in nervous affections of the larynx and trachea; *tannin* astringent and styptic, dose one to ten grains in an ounce of water; *alum*, one to twenty grains as above; *sesquichloride of iron*, five to twenty-five minims in an ounce of water; *acetate of lead*, three to ten grains as above; *sulphate of zinc*, one to ten grains; *common salt, tincture of opium, liquor arsenicalis, pure water, glycerine, lime water*, one part of lime to one hundred of water; and *cod-liver oil*. Dr Beigel further mentions the salts of iodine, chlorine, and bromine, as recommended by other authors.—*Dublin Quarterly Journal, Aug.* 1867, *p.* 116.

DISEASES OF THE ORGANS OF DIGESTION.

31.—ON DIPHTHERIA.

By Dr. JAMES ALDERSON, President of the Royal College of Physicians, Physician to St. Mary's Hospital.

I will first review the symptoms of the disease. The first setting-in very rarely comes within the observation of the physician. As far as we can tell, it always begins like an attack of severe cold accompanied with fever, and sore-throat. The illness progresses very rapidly, with a feeling of great prostration. When medical aid is first called in, the appearance of the throat varies much in different cases. In the greater number of them it is only a superficial ulceration of the tonsils; these are the mild cases, called for the name's sake diphtheria, but which I should call the sore-throat of mild scarlet fever, or in some instances quinsy; and which require the treatment proper to those complaints. Some of these cases terminate in suppuration. Upon the naturally to be expected recovery of these cases many of the reputations which are claimed for the cure of diphtheria are really founded. In many instances, however, the treatment by caustics and stimulants, which have been used in compliance with the idea that diphtheria especially required them, will have made only bad throats out of simple cases which would have subsided under gentler treatment.

It is not, however, of these simpler cases that I wish to speak, but of those of true diphtheria, in which the symptoms advance with a degree of rapidity which calls for prompt and vigorous measures. The real characteristics of this disease are the formation of a false membrane covering the tonsils and extending over the soft palate, and the quickness with which the membrane forms—threatening an extension to the pharynx and larynx. The patient at this period breathes with difficulty, and has no power to swallow. Liquids are immediately returned by the nose, and solid food of course is not attempted. The fauces are swollen, and in a state of what we used to call, and which I still call inflammation: it is a sort of inflammatory œdema, which is accompanied by the usual pain attending such a condition. There is a large secretion of mucus, and the the glands of the neck are frequently affected; there is always a quick small pulse: in short, every evidence of fever localised in the throat. This tumefaction of the throat, extending to the cellular tissue outside, with glandular enlargement, is the most unerring mark of internal lesion to a serious extent.

The specific character of diphtheria is thus described by M. Bretonneau; you will observe how it coincides with the symptoms of the case which you have seen so lately:—"At the beginning of the disease a circumscribed redness is perceived, covered with a semi-transparent mucus. This first layer, which is slight, supple, and porous, may be still further raised up by some portions of unaltered mucus, in such a manner as to form vesicles. Often in a few hours the red spots extend perceptibly from one to another, by continuity or by contact, like a liquid which is effused on a flat surface. The concretion becomes opaque, white, and thick; and it assumes a membranous consistence. At this period it is easily detached, and does not adhere to the mucous membrane, except by some very slender prolongations of concrete matter which penetrate into the muciparous follicles. The surface which it covers is generally of a light-red tint, with points of a deeper red. This tint is more vivid at the periphery of the spots. If the false membrane leaves the surface uncovered, the redness which was obscured by the exudation returns, and the points of a deeper red allow blood to transude. The concrete membrane is renewed, and becomes more and more adherent upon the points which had been first attacked. It often acquires a thickness of several lines, and passes from a yellowish-white colour to brown or black; and blood oozes out still more freely. All attempts to remove the membrane at this stage cause blood to ooze out freely."

Now, since we have seen that the one marked state of throat which tends so rapidly towards destruction is the formation of a false membrane, we ought to inquire how this membrane is formed, and to what morbid change it is to be attributed. It is my opinion that its first beginning must be referred to the inflamed—that is, gorged—state of vessels; and that this engorgement is only an aggravated condition of that same state of inflammation which, in a slighter degree, terminates in the milder form of sore-throat. The usual explanation of the existence of the membrane is that it is an exudation from the mucous glands and follicles. No doubt that is correct; still this statement affords no clue to the nature of the first stage of the disease, and, therefore, affords no instruction as to a rational mode of preventing its rapid progress. To my mind the gorged and inflamed vessels ought to be relieved in the first instance. As the progress is very rapid, it will of course not be possible to attempt this relief by any slow or gentle means; still the system of relief must be followed in opposition to that of irritation.

Topical depletion by the application of leeches has reason to commend it, and the testimony of success in all the cases in which I have tried it. Even after the formation of the membrane I have often seen the happiest results. The two cases which are the subjects of this lecture are both most successful instances;

but I shall refer to the particulars of these cases, with the accessory treatment, afterwards, as I wish now to insist on the broad principle of treatment by relieving the inflammatory state in the early stage, in opposition to that of irritants, stimulants, and tonics.

It is not desirable to continue the depletion longer than is called for to check the inflammatory action. The tendency in this disease to extreme debility and exhaustion is very great, but at the same time it has been most strikingly evident, in all cases which I have treated in this manner, that the rebound towards strength and convalescence succeeded immediately on relief attained through the leeches.

It seems a strange idea that return to a healthy condition of parts could be induced by setting up a fresh inflammatory action,—the application of strong caustics and stimulants and corroding acids being at the same time especially objectionable to a part so closely contiguous to the larynx, to which any extension of inflammatory action would be destructive to life.

The use of tonics, if less dangerous, has no reasonable excuse to recommend it. It seems, in fact, that to trust to this last class of remedies must be a mere delusion, because, in true diphtheria, as you have seen in the instance now before you, there is no power whatever to swallow; all attempts to pour down liquids merely result in their being returned through the nose. It is natural enough for surrounding friends to crave that the failing strength may be supported by nourishment and wine, and it is still more natural that they should believe the suggestions of medical attendants, that it is absolutely necessary to pour in sustenance; but it is worse than wasting time, in the early stage of the disease, to try to get in succour by a channel through which it could not possibly proceed. In fact, the patient is not in danger from want of food, but from the progress of the malady.

Whilst deprecating the recourse to caustics, it is but fair to state the theory on which those proceed who adopt that plan. The idea is to destroy the mouths of vessels which are in a diseased state, and to set up a new and healthier action. This is an old practice, founded on sufficient reason—a practice which, in indolent sores, where vital parts are not immediately concerned, has much success; but in this case, where the disease is in an active state, caustics applied to highly gorged and inflamed vessels must aggravate the diseased condition. Here, again, I must call your attention to the immediate proximity of vital parts, obviously rendering the use of caustics and stimulants a dangerous expedient, for the mere swelling and irritation of surrounding parts might be attended with serious consequences. Besides this the great temporary distress to the patient is much to be deprecated in the excited and febrile condition.

French works are full of specific treatment—viz., by strong hydrochloric acid and by caustics, with stimulants and tonics. I do not think the French sufficiently look back to a comprehensive classification of disease; they fly off to the consideration of new mysterious forms, and, having laid down certain dogmas as to treatment, bolster up their practice by unlimited assertions.

The reasons given by Bretonneau for the abandonment of the depletory system are at least curious. He gives, amongst others, two cases which are very significant:—

"A little girl 6 years old, of delicate constitution, had twenty-two leeches applied, at twice, with a short interval; the blood flowed abundantly for twelve hours. Two pounds of ipecacuanha syrup, with tartar emetic, were administered in the space of three days; after the sixth day, two grains of calomel were given every half hour, and during the night mercurial ointment was rubbed in under the arms and back of the neck. After twenty hours of slow asphyxia life was quietly extinguished. The last portions of calomel were not swallowed, and the skin had not sufficient warmth to dissolve the mercurial ointment.

"Another child, 6 years old, had twelve leeches applied, with the result of an abundant flow of blood and some relief to deglutition. Encouraged by this, a second application was ordered, and blood continued to flow for several hours. The following day, there was no longer any doubt that the air-passages were attacked by diphtheritic inflammation. The child died."

It would almost be an affront to point out to you this absurdity. I have no doubt you are already aware of the great danger of over-leeching children. Vigorous as I should myself desire to be in the endeavour to arrest the progress of croup, destructive measures such as these can scarcely be credited, were it not for the published testimony.

As an example of the effects of the application of corroding, concentrated acids, I quote the following case from the same work. The child was only eight months old, and several (four) of the same family had already died of diphtheria:—

"On the third day the back of the mouth was covered with a greyish incrustation. A sponge moistened with concentrated hydrochloric acid was applied to the isthmus of the throat, and two leeches to the mastoid processes. The same treatment was followed for three days, and the application of concentrated acid repeated night and morning. Death took place at the commencement of the sixth day. It was suspected that the applications of hydrochloric acid had been too frequent and too strong."

I need scarcely detail to you the line of practice that I adopt as the rational sequence of these my opinions regarding the nature of the disease. General depletion in diphtheria would be wholly out of place ; no one would think of it when collapse of power follows so rapidly on the inflammatory stage. But topical relief to the gorged state of the vessels by the moderate application of leeches is perfectly available. The leeches should not be placed too near the part affected, in order to avoid the consequence of effusion beneath the bites. A slight puncture of the membrane by a guarded lancet, with a view to give exit to the bloody serum and mucus, and to permit the ingress of a cleansing lotion, is also very useful. The lotion I generally use is a single grain of the sulphate of copper to eight ounces of water, applied as a gargle.

I would recommend the greatest care in handling the throat, and especially to avoid removing the membrane. It stands to reason that it must be an irritating measure to reduce the inflamed part to the state of an exposed abraded surface. It has always been observed that when the membrane is removed, a new one is immediately formed ; thereby the process of restoration having to be recommenced. I should deprecate the use of every irritating application in the early stage ; though when that gorged state of the parts, having an œdematous character, threatening immediate suffocation, has begun to subside, a certain amount of irritating applications may be admissible and even necessary. Febrifuge medicines are wholly futile, on account of the inability to swallow ; watching, however, for the turn of the disease, accompanied by the restored capacity to swallow, we may then have recourse to quinine and every form of support and moderate stimulus—proceeding, in short, as during recovery in fever. At this time caustics in the solid form may be needed : I say solid, in opposition to the more general applications, especially of strong hydrochloric acid ; since the solid caustic can be confined to the parts which are ulcerated.

A favourite and a much-followed plan which I have met with in private practice is to give ammonia, the excuse being to support the system. This is, in fact, nothing but stimulus in a form objectionable on account of its local action. Suppose you had a blister, and you were to remove the skin and to apply ammonia, what would you expect to be the result ?

I should include the use of chlorides, given with a view to depurate and deodorise, in the same category of dangerous applications. Chlorides immediately act upon the windpipe, and no amount of caution can render them otherwise than injurious.

The allusion I have just made to the similarity of treatment to that for fever reminds me once more to speak of the real character of the disease which we call diphtheria. In every case which I have seen I have been able to trace the actual presence of scarlet fever in the house or in its immediate neighbourhood.

I fully acknowledge that in diphtheria of the present day there seems to be this peculiarity, that the course of the attack proceeds with a rapidity and violence that have not been recorded in former times. This comparison, however, of the present with the past may be in a great measure exaggerated. We scarcely take into consideration the virulence of the old putrid sore-throat, of which there are records favouring a conjecture that it must have been as fatal as our present diphtheria; and in regard to the number of fatal cases at the present time I fear that, without being uncharitable, we may attribute many of them to the sort of procedure recorded by M. Bretonneau.

We must, however, not omit to notice certain distinctive signs in the sequelæ of diphtheria as compared with scarlet fever. In scarlet fever there is frequently albuminous urine and anasarca, and general dropsy. In diphtheria we have a local, sometimes even a general paralysis, though most frequently accompanied by albuminous urine. The albuminous urine attaches diphtheria very significantly to scarlet fever, the anasarca in the former and paralysis in the latter being distinctive. A closer investigation, however, greatly reduces this apparent difference. The anasarca belongs to the milder attacks of scarlet fever. It would, therefore, not be looked for in diphtheria, which we consider the most aggravated form. The paralysis of the throat which we find in diphtheria is to be referred in a great measure to loss of structure, and partly to excessive prostration. It is never permanent, but gradually disappears during the convalescence, and the recovery is assisted by time, tonics, and nourishment.—*Lancet, May* 4, 1867, *p.* 533.

32.—ON IODINE INHALATION IN THE TREATMENT OF DIPHTHERIA.

By Dr. J. Waring-Curran, Bexhill, St. Leonards.

[The first three cases which occurred in Dr. Waring-Curran's practice proved fatal. They were treated by free stimulation, cauterisation of the throat, and chlorine gargles.]

After an interval of several days, six new cases commenced, just as the consternation was beginning to subside, and when we had hoped the disease had left us; these I treated by local cauterisation with nitrate of silver where it was practicable, and the inhalation of iodine in hot vinegar and sage, prescribing large quantities of wine and ammonia, strong beef-tea, and ice, and counter-irritants over the chest, as in the first three. Some of these were truly formidable cases, manifesting symptoms which, writers on the subject tell us, once evinced, are never recovered from; but all ultimately recovered, with ten others which subsequently occurred. The wives and children of the artillery-men stationed in the Martello towers in the neighbourhood in several instances became affected; some deaths happened, and the disease only yielded to treatment when the doctor in attendance, at my suggestion, altered his plan, and had recourse to the iodine inhalation, which eventually turned out as efficacious with him as with me. Of its value in this disease I speak fearlessly, and truthfully consider that many of the cases I had under my charge must assuredly have succumbed had it not been for the service rendered by the iodine. To those to whose lot it may unfortunately fall the treatment of diphtheria, I would earnestly recommend a fair and early trial of inhalation of iodine combined with sage and hot vinegar.

The formula which I employ for inhalation purposes is the same as that formerly used by Sir Charles Scudamore in pulmonary phthisis—viz. : Iodine, iodide of potassium, of each four grains; alcohol, four drachms; water, four ounces. Of this, for each inhalation, commencing, I take a drachm; add to it a pint of vinegar infused with a handful of dried garden sage, placed in a common inhaling jar, steadily increasing the quantity of iodine solution until I arrive at half an ounce each inhalation. The circumstances of the case, the age and strength of the patient, and the severity or mildness of the attack, guide me with regard to the number of inhalations, and the time occupied by each. For an average case, occurring in a healthy patient, I would say twelve inhalations at least per diem, with eight to twelve minutes (an interval allowed to rest) for each. The loss of valuable time in country practice, occasionally, will not permit the delay of sending and waiting for an apparatus; accordingly not unfrequently I have been compelled to make my inhalation in the following manner:—Having boiled the vinegar and sage, place it in a teapot with a long spout, and when the patient is prepared to inhale, add the iodine, cover the lid of the vessel with a cloth, keep up the temperature by a spirit-lamp placed underneath, and holding the vessel by the handle, allow the patient to inhale through the spout. Laryngeal irritation is in a great measure prevented by the small quantity at first used, through the patient becoming gradually accustomed to it.—*Lancet, Sept.* 21 *and Oct.* 26, 1867, *pp.* 357, 513.

33.—ON DROPSY OF THE PERITONEUM: ASCITES.

By Dr. S. O. Habershon, Physician to Guy's Hospital.

The habit of designating disease by the name of one of its symptoms is fraught with many disadvantages; but this is, unfortunately, a practice too frequently adopted. Jaundice is merely a *symptom;* but it is the name given to a *class* of diseases, although produced by a variety of conditions. Albuminuria and Bright's disease are terms of general significance; so of others in ordinary use. Thus, also, dropsy of the peritoneum, or ascites, is often simply designated *dropsy;* and we admit that there is some excuse for applying such an appellation to this effusion into the largest of the serous membranes. Ascites is often associated with anasarca; and it thence constitutes a part of the general dropsy. But the varieties of peritoneal effusion are worthy of especial consideration; and it is to this *local* dropsy, in its several aspects, that I invite attention.

We distinguish several forms of peritoneal effusion.

1. From *atrophy:* as in senile wasting; in exhaustive cachexiæ; and in simple anæmia.
2. *Ascites from obstruction:* as in cirrhosis; in heart-disease; in chronic bronchitis; in any obstruction in the vena-portæ or vena cava.
3. Ascites from *renal* disease.
4. From *glandular disease*, whether affecting the spleen or lymphatic glands, &c.
5. *Inflammatory* ascites.
6. *Strumous* ascites.
7. *Cancerous* ascites.

And although in each of these forms of peritoneal effusion the malady may be produced by some general ailment affecting the whole system, in the last three we have a more especial *local* manifestation of disease; and I have found these varieties overlooked or imperfectly understood.

1. *Atrophic ascites* is often present at the close of wasting disease. We find it in the feebleness of old age; in exhaustive cachexiæ; and in simple anæmia. The effusion is of a passive kind; but it is sometimes sufficient to call for notice. The circulation in these cases is retarded, or has almost ceased, from failing power, or from fibrinous coagulation in the veins; and slow extravasation then takes place into the serous cavity and areolar tissue. It is a kind of exosmosis, and closely resembles the passage of serum into dependent parts of the body after the circulation has stopped.

2. A second form of ascites may be correctly designated *dropsy from obstruction.* Any mechanical impediment to the

passage of blood from the portal system of vessels produces this variety of ascites. In several instances I have seen cancerous disease extending directly into the inferior cava, and reaching to the right ventricle, thus preventing the exit of blood from the liver, and causing engorgement of the portal circulation; and in another instance this extension took place into the vena portæ itself, so that the whole structure of the liver was injected with cancerous product. In these rare cases ascites was present from mechanical hindrance to the course of the blood. More frequently we find obstructive ascites caused by chronic disease of the liver, or of the heart, or of the lungs and bronchi. In the latter varieties the legs become anasarcous, and so also in many cases of hepatic disease, as cirrhosis; for the obstruction affects the whole inferior cava, and in cirrhosis it will be found that contraction at the lobulus Spigelii hinders the free passage of blood from the inferior cava close to the heart. This form of effusion may be merely serous in its character, but, from the long-continued congestion of the capillary vessels of the peritoneum, the nutrition of the serous membrane is generally more or less interfered with; the membrane becomes thickened, granular, and in colour opaque; and, what is of still greater importance to remember (especially in the consideration of operative relief by paracentesis), acute inflammatory changes are very easily induced, and fibro-albuminous product is quickly poured out. I would, *en passant*, refer to a rare form of passive ascites from obstruction which I have witnessed on two occasions, in which the effusion had a milky aspect, and was in part of a chylous character. In one of these cases the pressure involved the thoracic duct; and in the other the mesenteric lacteal vessels were very much distended, from obstruction in the mesenteric glands.

3. A third form of ascites is that connected with *renal* disease. Acute albuminuria, whether following scarlet fever or from other cause is often accompanied by serious effuions into the peritoneum, as one of its symptoms, in common with general anasarca; and there is a great tendency to serous inflammation in this disease, apparently from the presence of urea in the serum. The peritoneum shares in this disposition; so that in an analysis of instances of peritonitis I found that, out of 500 fatal cases, 63 were in connexion with renal disease. These instances of ascites with general anasarca do not call for special treatment. As the renal affection subsides the fluid becomes absorbed; and the best mode of treatment is that directed to the relief of the original malady. But there are conditions in which we find renal disease with ascites without general anasarca: I refer to renal with hepatic disease, whether it be chronic contraction of the liver and kidney, as cirrhosis,

or lardaceous disease. The ailment is of a chronic kind, an the improvement very slow; whilst the ultimate recover depends on the state of the constitution.

4. A *fourth* variety of ascites might justly be called *glandular*. The peritoneal effusion is secondary to disease of important glands, by which the composition of the blood is changed. The ascites may be only part of a general dropsy. These instances arise from affections of the spleen and of the lymphatic glands, whether the change be one of congestion, of inflammation, of lardaceous, or other disease. The effusion is of a passive kind, and the treatment wholly of a constitutional character.

In other instances the ascites may be regarded as more especially of peritoneal origin, although the constitution is also at fault. The local treatment is of greater value than in the preceding forms of disease, for it is in the serous membrane that we have the manifestation of morbid action.—*Lancet, May* 4, 1867, *p.* 535.

34.—INTERESTING CASE OF PERSISTENT HEMORRHAGE FROM THE BOWELS, OCCURRING PERIODICALLY.

By Dr. M'Gregor, Medical Superintendent, Barnhill Hospital, Glasgow.

[The patient, who was 33 years of age, was seized four years ago by an attack of hemorrhage from the bowel, but with rest and astringents internally it shortly ceased. Since then she has had a similar attack every six or eight months, leaving her on every occasion in a pale and weakly state of health. On the last occasion of all, six months before this paper was written, the loss of blood became really alarming, the patient passing every night (always in the night) half or three-fourths an ordinary chamber-pot full of pure blood. This contined several nights in succession, till the patient had become so weak and prostrated, that it was clear if some relief was not obtained the hemorrhage must speedily prove fatal. No cause could be assigned whatever.]

The remedies, usually successful, had on this occasion no effect in checking the nightly recurring gush of blood. The patient was only kept alive by the liberal supply of brandy, wine, and beef-tea, along with light and nutritious articles of diet. Large quantities of ice were used to check the vomiting that was now setting in from sheer exhaustion and loss of blood, there being also vertigo, cold clammy sweats, and other symptoms of collapse present.

In this way the case went on for more than three weeks, the hemorrhage re-appearing every time the patient rallied, and reaction set in. She was perfectly conscious of her condition, though extremely weak, and could always tell when a fresh gush of blood might be expected by *hearing* and *feeling* it accumulating within the abdomen with a rushing noise from each of her sides towards the middle of the belly, about the umbilicus. Immediately upon hearing the gurgling noise, a feeling of pain, swelling, and uneasiness set in, pressing so much on the stomach and other parts as to cause vomiting, and an urgent desire to go to stool. No rest could be obtained till this desire was yielded to, when blood in alarming quantities would gush in a stream from the bowel, unmixed as a rule with any excrementitious matter. Instant relief was obtained on its evacuation, and the patient continued easy till the same process again set in on the following night, attended by symptoms in all respects similar to the preceding attack.

Several medical gentlemen of standing saw the patient with me, and every remedy that could be suggested was put in use. Astringent injections of various kinds were employed, including perchloride of iron and astringent powders and mixtures by the mouth. Every position and posture that was likely to be of use, such as raising the pelvis by placing pillows below it, &c., were tried, but all were of little avail so far as checking the ever recurring hemorrhage was concerned. Strange to say, the only position that seemed to exercise any marked control over the quantity of blood passed was placing the patient on her elbows and knees. She was convinced herself that not only were the intervals between the attacks thus lengthened, but the quantity of blood was not so large when at any time she felt able to maintain this position. But she was so weak as to render it impossible for her to remain long in so awkward a posture, so that its prolonged effects could not be decided on.

Having at last despaired of finding a remedy or discovering a solution of the cause of such unusual symptoms, I was advised by my friend, Dr. Thomas, of Glasgow, to give the secale cornutum a trial, as he had noticed good effects from it in some cases of hemorrhage from the bowel.

I had little faith in the ergot after so many other agents had failed, but thinking at least that no harm was likely to arise from its use, I determined to try it, and began with half dram doses every four hours.

The patient had had an attack the previous night of the usual character, and there were no signs whatever of an amendment when the ergot was begun. From that day to this, not a drop of blood has been lost to the patient's knowledge. The ergot was continued, the same as above, for a week. Then for a second week in the same dose three times a-day, when it was stopped, and tincture of steel with other restoratives given instead. The patient has gone on gaining more and more strength ever since, and has been at her usual employment for several months.

Whether the hemorrhage will recur after a few months, as it has repeatedly done before, or whether in the event of its doing so ergot of rye will prove successful in subsequent attacks, is a matter of equal uncertainty; but I think there can be very little doubt that the cessation of the bleeding in this attack was decidedly due to the use of the ergot.

It would be interesting to know exactly the cause of the hemorrhage as well as the modus operandi of the drug just named in this particular case.

It is but right to state that the patient has shown recent symptoms of incipient phthisis, but the history of the case would indicate phthisis to be the effect rather than the cause of the hemorrhage. For it is reasonable to suppose that such repeated loss of blood for several years was calculated to lower the system and impoverish the blood. Even allowing abdominal phthisis to be the cause, it would be difficult to reconcile the repeated attacks at long intervals, extending over four years, with softening of tubercle or glandular ulceration of the bowels, without at least other evident symptoms of mischief developing themselves before now, which is not the case.

The patient is somewhat of a relaxed habit, and never was very robust, but there were no other symptoms of the hemorrhagic diathesis, usually so-called, in her case.

It would seem to be due rather to a relaxed state of the capillaries, occurring periodically as the result of some want of tone in the nervous system, and it is likely, I think, that the ergot must have acted by a direct influence on the blood vessels counteracting this defect in the nervous system, or it acted on the nerves themselves, leading to the same result. It is well known that ergot has a powerful effect in lessening congestion of the spinal cord, and it is very likely that it does so by its influence on the nervous system, regulating the vascular supply by contracting the blood vessels, and thus lessening the amount of blood to the part. It is probable that it acted in a similar manner here, by giving tone to relaxed muscular fibre directly, or by its effect in the nervous system regulating the vascular, in such a way as to stop the hemorrhage.

I shall note the future history of the patient if possible, and report a recurrence of the bleeding, with the result of treatment by the ergot in any subsequent attack. Meantime I would recommend a trial of the ergot in frequent doses in obstinate bleeding from the bowels, especially if the ordinary remedies fail to arrest it, as they did in the present instance.

Note.—The above account was written in April last year. The patient is at this date quite well, and stronger than I have seen her for a couple of years past. The bleeding has not since troubled her.—*Glasgow Medical Journal, June* 1867, *p.* 46.

35.—DR. MARCET'S PROCESS OF ARTIFICIAL DIGESTION.

We direct the attention of our readers to a pamphlet by Dr. Marcet which, like everything emanating from the same source, contains matter well worth general attention. Dr. Marcet first alludes to the importance of supplying nourishment in sufficient quantity to the system in cases where, from some peculiarity, the stomach is incapable of fulfilling its functions aright. Beef-tea and milk are often the only articles of diet the stomach will bear, and even the latter frequently disagrees, whilst beef-tea alone does not supply sufficient pabulum. It therefore struck Dr. Marcet that the process which naturally goes on in the stomach might be imitated artificially, and so supply the food in a partially digested condition. He, accordingly, took hydrochloric acid and some pepsine, added these along with water to a quantity of meat, allowing the whole to simmer over a water-bath at about the temperature of the body. When the meat was sufficiently broken up, it was strained and the acid neutralised by carbonate of soda, when it was ascertained that the product was of a most agreeable character, easily digestible, and containing a vast deal more nourishment than common beef-tea. The proportions he recommends are 58 grains of hydrochloric acid, sp. gr. 1·1496, in a pint (20 oz.) of water, with 15 grains of Boudault's pepsine, and 81 grains of bicarbonate of soda to a pound of meat (weighed raw), the chemicals costing about sevenpence. Where pepsine is unattainable, strips of calves' stomachs answer very well, or we do not see why the rennet prepared from it and used for curdling milk should not be employed. The food thus prepared keeps well until neutralised, but not so well afterwards. One point to be noticed is that no metallic vessel should be used in the process, lest the acid act upon it, but for full particulars we must refer our readers to Dr. Marcet's little work.*—*Medical Times and Gazette, June* 8, 1867, *p.* 633.

36.—VOMITING, WITH SARCINÆ VENTRICULI.

[The following short paragraph is taken from a report on the practice at University College Hospital.]

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* "On a New Process for Preparing Meat for Weak Stomachs." By W. Marcet, M.D., etc. London: John Churchill and Sons.

For these vegetable growths the sulphite, bisulphite, or hyposulphite of soda is commonly used, but Dr. Reynolds prefers using the pure sulphurous acid. He recommends the sulphurous acid to be made in the ordinary way, and to be passed through water until the latter has dissolved up as much as it will take. This saturated solution is to be diluted until the acid is no longer disagreeable to swallow, when an ounce should be given before each meal. The sarcinæ may be thus destroyed; yet they are apt to return, as will other parasites, until the conditions on which their presence depends have been removed.—*Medical Times and Gazette, July* 27, 1867, *p.* 91.

DISEASES OF THE URINARY ORGANS.

37.—ON VARIOUS POINTS OF ANALOGY BETWEEN THE LIVER AND KIDNEY AS REGARDS THEIR STRUCTURE, FUNCTIONS, AND PATHOLOGY.

By Dr. George Johnson, Professor of Medicine in King's College.

The diseases of the *kidney* are much better understood, as regards their morbid anatomy, their pathology, and their diagnosis, than those of the liver. There are several reasons for this: 1st, the structure of the kidney is more simple than that of the liver; 2nd, its secretion can readily be obtained for chemical and microscopical examination; and 3rd, most of the morbid materials are formed within the uriniferous tubes, and thus escape with the urine. So that the nature and the stage of each disease may commonly be determined with great accuracy during life.

There are analogies as to structure, and function, and pathological conditions between the liver and the kidney, and a knowledge of renal pathology affords much assistance in clearing up some obscure points relating to diseases of the liver.

Let me give you some examples of analogy between the liver and kidney. In both organs the secreting cells are the essential elements of the gland, the agents by which the materials of each secretion are separated from the blood. In both organs the gland-cells are contained within a tubular membrane. This is quite obvious in the kidney; and no unprejudiced observer who has examined Dr. Beale's specimens can doubt that the ducts are continued into the lobules of the liver, and that the cells are contained within the tubes. There is within the lobules "a cell-containing network" of tubes.

One difference between the two glands is that the bile is secreted chiefly from venous blood, the urine from arterial blood. Yet again, there is this remarkable resemblance between the liver and the kidney, that both the bile and the urine are secreted from blood which has previously passed through one set of capillary vessels. The blood of the portal vein has passed through the capillaries of various abdominal organs before it enters the liver. The blood of the inter-tubular renal plexus has passed through the Malpighian capillaries within the kidney.

Each efferent vessel is, as Mr. Bowman states (in his original paper, "On the Structure and Use of the Malpighian Bodies of the Kidney" in the "Philosophical Transactions,") *truly a portal vein in miniature*, and it resembles the portal vein in the fact that it intervenes between two capillary networks, the first of which answers to that in which the vena porta originates, and the second to that in which the vena porta terminates.

One interesting question relating to the secretion of bile has been much debated amongst physiologists and pathologists. It is this : Do the constituents of the bile, or any part of them, exist ready formed in the blood, or are they formed entirely by the liver? Some physiologists maintain that the bile is formed exclusively by the liver. Their doctrine is that the liver-cells withdraw from the blood certain materials, which they separate into bile and glucose or sugar ; the bile then passes into the ducts, and the sugar into the blood of the hepatic vein. In opposition to this doctrine, it is asserted that some, at least, of the bile-constituents exist ready formed in the blood, and that the office of the liver-cells is mainly to separate these materials from the blood during its transit through the gland. This question is one which concerns, not only the physiologist but also the pathologists and the practitioner. Our views as to the pathology and diagnosis, and even the treatment of certain forms of liver disease must be influenced by the opinions which we entertain upon this physiological question.

I will briefly refer to the main facts and arguments in favour of either view. The analogy of secretion by the kidney is in favour of the doctine that some at least of the bile-constituents exist ready formed in the blood, and require only to be separated by the liver-cells. The chief of the urinary solids—urea and uric acid—are known to exist in the blood. The kidney *secretes or separates*, but does not *form* them. And when the kidneys are destroyed by disease in the human subject, or when in the lower animals the glands are extirpated or their functions suspended by ligature of the artery or duct, death speedily results from an accumulation of urinary constituents in the blood.

In opposition to this argument from the analogy of the renal secretion, when applied to the secretion of bile, it is asserted that chemistry fails to detect in healthy blood any bile-constituents. But this failure to detect bile-constituents in the blood is no proof of their non-existence. Bile is continually reabsorbed from the intestines, and returns into the circulation; yet chemistry fails to detect it in the blood. A chemical examination of jaundiced blood finds only the biliary colouring-matter, and not the biliary acids. And, further, it is acknowledged by Frerichs that even when large quantities of bile have been injected into the blood, so as to obviate the difficulties which there might be in detecting small quantities, there is an equal want of success; the colouring-matter only can be detected. The biliary acids appear to undergo some rapid transformation; at any rate they escape detection. On chemical grounds, therefore, there is no reason for rejecting the theory which assumes that some at least of the constituents of bile—the colouring-matter, if not the bile acids—pré-exist in the blood, and require only to be separated by the liver-cells.

Most physiologists agree in the opinion that there is a close and intimate relationship between the colouring-matter of the bile and urine and that of the blood. It is probable that the two former may be derivatives from the latter.

The doctrine of the formation of bile exclusively by the liver has led to some fanciful hypotheses as to the pathology of jaundice occurring without any apparent obstacle to the escape of bile through the ducts. For instance, jaundice has frequently resulted from obstruction of the *portal vein.* How is this to be explained on the assumption that bile is formed only in the liver? An impeded flow of blood to the liver should result in a *lessened formation* of bile, and not in any accumulation in the blood. Frerichs explains the jaundice in these cases by what he calls "abnormal diffusion." "In consequence of obstruction of the main trunk, or of the larger branches of the portal vein, the tension of the capillary vascular system is diminished, and the entrance of the biliary contents of the hepatic cells into the blood is facilitated." This appears to me a very clumsy, mechanical, and unsatisfactory explanation. On the other hand, admit—what cannot be disproved—that at least the colouring constituents of the bile pre-exist in the blood, and the jaundice which results from obstruction of the portal vein receives a ready explanation. In consequence of the obstructed flow of blood there is suppressed secretion, and therefore an accumulation of biliary materials in the blood: just as from ligature of the renal artery there occurs suppressed secretion of urine.

I am decidedly of opinion that the weight of evidence preponderates on the side of those who maintain that some of the bile-constituents, like those of the urine, exist ready formed in the blood, and that jaundice may result, as Dr. Budd teaches, 1st, from an impeded escape of bile through the ducts, and its consequent reabsorption into the blood; or 2ndly, from suppressed secretion consequent on impeded circulation, or destructive changes in the secreting cells. It is probable that as the bile is in part a mere secretion and in part an actual formation by the liver, so the urinary constituents are in part formed by the kidney. This, at any rate, is rendered probable by an experiment performed by Dr. Oppler, of Berlin, who found after ligature of the uterus of a dog much more urea in the blood and muscles than after extirpation of the kidneys: and he believes that this excess is due to the actual formation of urea by the kidneys; so that they not only separate urea which has been ready formed, but they have also a large share in forming it. With reference to this experiment, however, it would be important to know whether the dogs whose kidneys were extirpated did not die sooner than those whose ureters were tied. If so, the excess of urea in the blood and tissues after ligature of the ureters might be simply a result of the greater duration of life after the function of the kidneys had been suspended.

In opposition to the theory of the pre-existence in the blood of some of the bile-constituents, and to the theory of jaundice from suppression, it is argued that jaundice would be a more constant result than it is of extensive degeneration of the secreting tissue of the liver, such as occurs, for instance, in the advanced stages of cirrhosis.

To this argument the reply is, that jaundice in a greater or less degree *is* a common result of advanced cirrhosis; and its frequent occurrence in these cases, when there is no evidence of impediment to the escape of bile through the ducts, is more difficult of explanation on the hypothesis of bile-formation exclusively by the liver, than is its occasional absence on the hypothesis of the pre-existence of some bile-constituents in the blood.

The observation is common to the two organs—the liver and the kidney—that a *slow* degeneration and obstruction of their secreting tissues may reach a very advanced stage without giving rise to any of the ordinary symptoms of suppressed secretion. As cirrhosis of the liver occurs without jaundice, so atrophic granular degeneration of the kidney may reach an advanced stage without dropsy or signs of uræmia. This is partly accounted for by vicarious elimination through other organs. Biliary materials are excreted by the kidneys, urinary excreta by the liver, and both by the intestines, and perhaps in a less degree by the skin. In these cases sudden suppression occasionally happens as a result of some additional disturbing cause. A superabundant or indigestible meal causes suppression of bile and jaundice, in the case of cirrhosis. Exposure to cold induces suppression of urine, dropsy, and uræmia in the case of advanced degeneration of the kidney. In some of these cases there has been no suspicion of serious disease until within a few hours or days of the fatal termination.

It is probable that some of the cases which have been recorded as examples of *acute atrophy of the liver*—cases in which the liver is supposed to have shrunk to one-half or one-third of its original size within a few days—have, in reality, been cases in which a slow atrophy and disorganisation of the gland have been insidiously making progress long before the sudden development of the serious symptoms which have been supposed to indicate the actual commencement of the hepatic disease.

[It is in the discharge of their functions as excretory glands that the liver and the kidney, in most instances, become diseased, and thus disease in both occurs most frequently in the *secreting cells.*]

The secreting cell is the essential part of the gland—its most vitally active and sensitive part; and the secreting cell is the first structure to show a deviation from the normal condition when morbid materials are conveyed to the gland through the blood.

There is much that is mysterious in the act of secretion, as in every vital process; but it is a well established fact that certain cells exert a kind of affinity for particular materials which are conveyed to them by the blood, and, by virtue of this affinity, the cells first abstract the materials from the blood, and subsequently discharge them into the ducts of the gland. The cells of each gland are endowed with the power of excreting certain products rather than others: the liver-cells secrete bile, and the kidney-cells urine. We know little of the precise nature of this power; but we see clearly that it is associated with a particular form of cell. The normal appearance of the gland-cells is as peculiar and characteristic as are the physical and chemical properties of their respective secretions. The cells of the kidney and the liver, while they are alike in all particulars which constitute them gland-cells, yet differ from each other as much as do their respective secretions—as much as urine differs from bile. It is evident, then, that there is an intimate relation between the structure and appearance of the cells and the nature and composition of their secreted products.

The cells of each gland, besides the constituents of their own proper secretion, have also the power of separating from the blood certain materials which do *not* form a part of their normal secretion.

Many inorganic salts and many odorous and colouring matters derived from the vegetable and animal kingdoms, when introduced into the blood through the stomach, are readily eliminated by the kidneys, and apparently without injury to the secreting cells of the gland. The elimination of other substances is attended with an alteration of the structure and appearance of the cells. For instance, the long-continued secretion of sugar in cases of diabetes generally induces changes in the renal gland-cells. In these cases the epithelium of the kidney becomes opaque, and often contains a large amount of finely granular material with oil. In some instances the urine becomes albuminous, and contains casts of the uriniferous tubes. The persistent secretion of sugar by the kidneys changes the appearance and the vital properties of the renal cells, so that, in adapting themselves, as it were, for the secretion of sugar, they become gradually unfitted for the secretion of urine. The circulation through the gland is impeded; then the kidney becomes congested and the urine albuminous. Suppression of urine is not unfrequently the immediate cause of death in cases of diabetes. The injury which the secreting cells of the kidney sustain in consequence of having to eliminate for a long period large quantities of sugar is one amongst other weighty reasons for keeping a diabetic patient upon a diet which shall tend as much as possible to lessen the formation of sugar.

In cases of jaundice, some of the biliary constituents are excreted by the kidneys; but during the process of eliminating these new products, the nutrition of the renal cells becomes modified, and the cells are often shed by a process of desquamation. Sometimes, too, the urine contains albumen and tube-casts, in consequence of the congestion of the kidney, which results from the presence of bile in the blood, and its slow and difficult elimination by the renal gland-cells. A short time since I attended a gentleman whose urine for a few hours contained a very large amount of bile, consequent upon obstruction of the gall-duct by a calculus. So long as the urine contained bile, it also contained a large amount of albumen. As soon as the gall-stone passed, the bile resumed its usual course; and as soon as the kidney ceased to secrete bile, the urine ceased to be albuminous.

Various morbid poisons, or the products of morbid poisons, exert a similar influence on the renal epithelium while they are being excreted—e. g., the poison of scarlatina, diphtheria, and erysipelas.

The cells of the liver are liable to precisely analogous changes under the influence of various morbid materials which are conveyed to them by the blood. These changes in the liver-cells, however, cannot, like those in the kidney, be recognised during the life of the patient, and even after death they are less easily demonstrated in the liver than in the kidney; yet they can be shown to occur, and the recognition of these changes in the gland-cells affords the only means of truly interpreting the pathology of the liver and the kidney.

There is a very close resemblance between the diseases of the liver and those of the kidney, comparing them only as regards their general outward character.

A. Fatty degeneration occurs in both, and in both much of the fat accumulates in the gland-cells.

B. Both organs are liable to the same forms of inflammation.

1. Suppurative inflammation is more common in the liver, chiefly because the blood of the portal vein is especially liable to be contaminated by morbid fluids from ulcers in the intestines, &c., and by the products of inflamed hemorrhoidal and other veins.

2. There is an acute and a chronic form of inflammation which is *not suppurative*, and which in the kidney I have called *desquamative*.

C. That remarkable form of degeneration which has been called *scrofulous*, *lardaceous*, *waxy*, or *amyloid*, occurs in both organs, and has essentially the same characters in both.

D. The so-called fibrinous deposits occur in both, and these deposits are liable to undergo a fatty transformation, and thus give rise to an appearance which has been called local fatty degeneration of the liver and kidney.

E. Cancerous and hydatid tumours occur in both organs; but these are both more common in the liver than in the kidney.

F. Lastly, concretions may be formed out of the secretion of each organ: gall-stones in the liver; urinary calculi in the kidney.

Postscript.—A correspondent has directed my attention to the statement which I have made in the early part of this lecture, "that both the bile and the urine are secreted from blood which has passed through one set of capillary vessels." And he objects that this statement is in opposition to Mr. Bowman's theory, that the liquid portion of the urine is derived mainly from the Malpighian capillaries. What I intended to say was, that the *peculiar solids* of the urine are secreted from blood which has previously passed through one set of capillaries —namely, those of the Malpighian tuft. It is probable that the urine, as it is secreted by the convoluted tubes, has nearly the same consistence as the bile, and that this thick liquid is diluted by the water, which is continually poured from the Malpighian capillaries into the extremities of the tubes.—*Lancet, August* 24 *and* 31, 1867, *pp.* 221, 256.

38.—ON THE CURABILITY OF BRIGHT'S DISEASE.

By Dr. Arthur Hill Hassall, Senior Physician to the Royal Free Hospital.

[Dr. Hassall has for some time been engaged in investigating cases of Bright's disease in order to determine the extent of the aid in diagnosis, prognosis, and treatment, to be derived from a careful and rigorous microscopical and chemical examination of the urine. In the first case the microscopical examination of the urine showed that though the case was granular degeneration of the kidneys and therefore serious enough, yet that since it was not one of fatty degeneration there was a fair prospect of amendment, if not of absolute recovery. The mean amount of albumen lost per diem was 70·4 grains, but under the treatment adopted, in two months it was only 4·86 grains. The next case was one of fatty degeneration of the kidneys, and the amount of albumen excreted in the twenty-four hours was reduced in nine months, during which the treatment could hardly be said to be very thoroughly carried out owing to the man's own imprudences, from 310 to 163 grains daily. Many other interesting cases are related, and the success attending their treatment seemed to hinge upon the presence or absence of *fatty* degeneration.]

The treatment adopted in all the above cases, although varying in details, was similar in its principles. The free action of the skin was maintained by the occasional use of the vapour bath. Congestion of the kidneys, when present, was relieved by means of dry cupping. Tonics and astringents were administered, consisting chiefly of the tincture of the sesquichloride of iron, sulphate of quinine, tannic and gallic acids, and when the effusion was very great, occasional doses of hydragogue cathartics and diuretics were given; but above and before all, great attention was paid to the diet. This was rendered highly nitrogenous; milk and eggs were freely given, and meat two and even three times a day; indeed it is to the diet prescribed, and the rest from toil and anxiety experienced by the patients while remaining in the hospital, that I chiefly attribute the successful results of the treatment. The absence of wasting, notwithstanding the enormous loss of albumen, is remarkable in most cases of Bright's disease, and was especially noticed in the cases above recorded, the majority of the patients throughout their illness retaining a considerable degree of plumpness and flesh.

The results obtained by the treatment of the several cases, so far as the albumen contained in the urine is concerned, stand thus:—

Case 1.—Francis S. In this case the mean amount of albumen excreted, after his admission into the hospital on the 20th of October, 1864, was 70 grs. per day, but when he left at the end of January, it was reduced to 4·86 grs.

Case 2.—Edward G. The mean quantity of albumen in the urine at the period of his admission on October 29th, 1864, was 310 grs. per day, which was reduced on quitting the hospital, in the early part of January, to an average of 195 grs.; but at the end of July a still smaller quantity was found—namely, 161·4 grs., showing a total diminution of 148·6 grs.

Case 3.—Joseph J. This patient, shortly after his admission on April 19th, 1865, was passing 357 grs. of albumen per day, and on leaving the hospital of his own accord the amount still remained high, being 362 gr., notwithstanding which his condition had greatly improved; but at the end of July the mean quantity was only 202·9 grs., being a reduction of 154·1 gr.

Case 4.—John C. The average daily amount of albumen passed after admission on Jan. 12th, 1865, was 184·05 grs.; this was considerably decreased by the end of March, when he was discharged from the hospital, 124·4 grs. being then excreted. This quantity, however, was still further reduced when the urine was next examined, on Aug. 5th, 77 grs. being found, showing a diminution of 107 grs.

Case 5.—Martin H. In this case the patient, at the period of his admission on Feb. 15th, was excreting 245 grs. of albumen daily; but on July 9th, just before he was sent to the Convalescent Institution, only 6·16 grs. were found for the day, the patient being in fact almost completely cured.

I must confess that the very striking facts recorded in this paper have greatly surprised me. The impression I had derived from books in reference to Bright's disease was that it was nearly always a fatal and incurable affection, whereas it really is much more amenable to treatment than could have been anticipated—so much so, indeed, as in my belief to fully justify the title I have given to this communication. The particulars recorded appear also to prove that the disease is not exclusively local and organic, but is to a great extent a blood disease.—*Lancet, Dec.* 18, 1865, *p.* 670.

SURGERY.

BONES, JOINTS, ETC.—AMPUTATIONS, FRACTURES, DSILOCATIONS.

39.—ON A NEW METHOD OF TREATING COMPOUND FRACTURE, ABSCESS, &c.

By JOSEPH LISTER, Esq., F.R.S., Professor of Surgery in the University of Glasgow.

[In our last volume we printed Prof. Lister's former papers on this subject. The *method of treating abscess*, described in the following article, has been found extremely satisfactory.]

It is based, like the treatment of compound fracture, on the antiseptic principle, and the material employed is essentially the same—namely, carbolic acid, but differently applied in accordance with the difference of the circumstances. In compound fracture there is an irregular wound, which has probably been exposed to the air for hours before it is seen by the surgeon, and may therefore contain in its interstices the atmospheric germs which are the causes of decomposition, and these must be destroyed by the energetic application of the antiseptic agent. In an unopened abscess, on the other hand, as a general rule, no septic organisms are present, so that it is not necessary to introduce the carbolic acid into the interior. Here the essential object is to guard against the introduction of living particles from without, at the same time that a free exit is afforded for the constant discharge of the contents. The mode in which this is accomplished is as follows :—

A solution of one part of crystallized carbolic acid in four parts of boiled linseed oil having been prepared, a piece of rag from four to six inches square is dipped in the oily mixture, and laid upon the skin where the incision is to be made. The lower edge of the rag being then raised, while the upper edge is kept from slipping by an assistant, a common scalpel or bistoury dipped in the oil is plunged into the cavity of the abscess, and an opening about three-quarters of an inch in length is made, and the instant the knife is withdrawn the rag is dropped upon the skin as an antiseptic curtain beneath which the pus flows out into a vessel placed to receive it. The cavity of the abscess is firmly pressed, so as to force out all existing pus as nearly as may be (the old fear of doing mischief by rough treatment of the pyogenic membrane being quite ill-founded); and if there be much oozing of blood, or if there be a considerable thickness of parts between the abscess and the surface, a piece of lint dipped in the antiseptic oil is introduced into the incision to check bleeding and prevent primary adhesion, which is otherwise very apt to occur. The introduction of the lint is effected as rapidly as may be, and under the protection of the antiseptic rag. Thus the evacuation of the original contents is accomplished with perfect security against the introduction of living germs. This, however, would be of no avail unless an antiseptic dressing could be applied that would effectually prevent the decomposition of the stream of pus constantly flowing out beneath it. After numerous disappointments, I have succeeded with the following, which may be relied upon as absolutely trustworthy. About six teaspoonfuls of the above-mentioned solution of carbolic acid in linseed oil are mixed up with common whitening (carbonate of lime) to the consistence of a firm paste, which is in fact glazier's putty with the addition of a little carbolic acid. This is spread upon a piece of sheet block tin about six inches square; or common tinfoil will answer equally well if strengthened with adhesive plaster to prevent it from tearing, and in some situations it is preferable, from its adapting itself more readily to the shape of of the part affected. The putty forms a layer about a quarter of an inch thick; it may be spread with a table-knife, or pressed out with the hand, a towel being temporarily interposed to prevent the putty from sticking to the hand or soiling the coat-sleeve. The tin thus spread with putty is placed upon the skin so that the middle of it corresponds to the position of the incision, the antiseptic rag used in opening the abscess being removed the instant before. The tin is then fixed securely by adhesive plaster, the lowest edge being left free for the escape of the discharge into a folded towel placed over it and secured by a bandage. This dressing has the following advantages:—The tin prevents the evaporation of the carbolic acid, which escapes readily through any organic tissue such as oiled silk or gutta-percha. The putty contains the carbolic acid just sufficiently diluted to prevent its excoriating the skin, while its substance serves as a reservoir of the acid during the intervals between the dressings. Its oily nature and tenacity prevent it from being washed away by the discharge, which all oozes out beneath it as fast as it escapes from the incision; while the extent of the surface of the putty renders it securely antiseptic. Lastly, the putty is a cleanly application, and gives the surgeon very little trouble; a supply being daily made by some convalescent in an hospital, or in private practice by the nurse or a friend of the patient; or a larger quantity may be made at once, and kept in a tin canister. The dressing is changed, as a general rule, once in twenty-four hours; but if the abscess be a very large one, it is prudent to see the patient twelve hours after it has been opened, when, if the towel should be much stained with discharge, the dressing should be changed, to avoid subjecting its antiseptic virtues to too severe a test. But after the first twenty-four hours, a single daily dressing is sufficient. The changing of the dressing must be methodically done, as follows:—A second similar piece of tin having been spread with the putty, a piece of rag is dipped in the oily solution, and placed on the incision the moment the first tin is removed. This guards against the possibility of mischief occurring during the cleansing of the skin with a dry cloth and pressing out any discharge which may exist in the cavity. If a plug of lint was introduced when the abscess was opened, it is removed under cover of the antiseptic rag, which is taken off at the moment when the new tin is to be applied. The same process is continued daily till the sinus closes.

The results of this treatment are such as correct pathological knowledge might have enabled us to predict. The pyogenic membrane has no innate disposition to form pus, but does so only because it is subjected to some preternatural stimulus. In an ordinary abscess, whether acute or chronic, the original cause that led to suppuration is no longer in operation, and the stimulus that determines the continued pus formation is derived from the presence of the pus pent up in the interior. When an abscess is opened in the ordinary way this cause of stimulation is removed, but in its place is substituted the potent stimulus of decomposition. If, however, the abscess be opened antiseptically, the pyogenic membrane, freed from the operation of the previous stimulus without the substitution of a new one, ought, according to theory, to cease to suppurate, while the patient should be relieved from any local or general disturbance caused by the abscess, without the risk of irritative fever or hectic.

Such, accordingly, is the fact. Abscesses of large size have, after the original contents have been evacuated, furnished no further pus whatever, the discharge being merely serum, which in a few days has amounted only to a few drops in the twenty-four hours. Whether the opening be dependent or not is a matter of perfect indifference, the small amount of unirritating fluid being all evacuated spontaneously by the rapidly contracting pyogenic membrane. At the same time, we reckon with perfect certainty on the absence of all constitutional disturbance.

As an illustration, I may mention the last case which has come under my care. It is that of a young woman, 25 years old, with psoas abscess, which had of late been rapidly on the increase, and caused a large swelling below Poupart's ligament, communicating with a fluctuating mass, dull on percussion, reaching to a considerable distance up the abdomen, the femoral vessels being raised over the communication between them. Six days ago I opened, in the manner above described, the swelling in the thigh at the anterior part of the limb where it was nearest the surface, giving exit to twenty-seven ounces of pus, thin, but containing numerous large curdy-masses. I introduced a piece of lint, dipped in the carbolic acid and oil, into the incision; and this prevented any discharge from escaping during the next twenty-four hours, when, on removal of the plug of lint under an antiseptic rag, three ounces turbid serum escaped. For the next three days there was scarcely any discharge, the deeper parts of the incision having cohered. On firm pressure, however, the product of seventy-two hours escaped, and amounted to four drachms of serum. Meanwhile the girl's general health, which had not been interfered with by the abscess, continued perfectly good, neither pulse, tongue, appetite, nor sleep having been disturbed.

In this case, though there is no deformity of the spine, there is great probability that caries of the vertebræ is present. But even though such be the case, there is good reason to hope for a favourable issue. Regarding caries as merely the suppurative stage of chronic inflammation in a weak form of tissue, I have been not surprised, though greatly rejoiced, to find that it exhibits the tendency of inflammatory affections generally—viz., a disposition to spontaneous cure on the withdrawal of irritation. Hitherto, in surgical practice, caries has had to contend against the formidable irritation of decomposing matter, which, under circumstances of weakness, is often sufficient to cause ulceration, even in the soft parts; yet, in spite of this irritation, caries is often recoverable in the child where the vital powers of all the tissues are stronger. If, therefore, this serious complication can be avoided, there seems nothing in theory against the probability that caries may prove curable in the adult. And even should portions of necrosed bone be present, as is not unfrequently the case, our experience of the treatment of compound fracture with carbolic acid has taught us that dead bone, if undecomposed, not only fails to induce suppuration in its vicinity, but is liable to absorption by the granulations around it.

Such were the hopes which I ventured to express several months ago to my winter class. Since that time I have opened numerous abscesses connected with caries of the vertebræ, the hip, knee, ankle, and elbow, and in all cases I have found the discharge become in a few days trifling in amount, and in many it has ceased to be puriform after the first twenty-four hours. Finally, three days ago—viz., on the 4th inst., (July, 1867,) I had the inexpressible happiness of finding the sinus soundly closed in a middle-aged man, in whom I opened in February last a psoas abscess, proved to be connected with diseased bone by the discharge, on one occasion, of an osseous spiculum. For months past we had persevered with the antiseptic dressing, although the discharge did not amount to more than a drop or two of serum in the twenty-four hours, well knowing by bitter experience that so long as a sinus existed the occurrence of decomposition might produce the most disastrous consequences; and at length our patience has been crowned with success.

Hence I no longer feel any hesitation in recommending the early opening of such abscesses, because, while they remain unopened, the disease of the bone is necessarily progressive, whereas when opened antiseptically, there is good ground to hope for their steady, though tedious, recovery.

The putty of the strength above recommended, though it generally fails to excoriate the skin, sometimes produces this effect when long continued. In such case it may be reduced in strength so that the oil contains only one part to five or six without disadvantage when the discharge is very small in amount.

The application prevents the occurrence of cicatrisation in the little sore caused by the incision, and perpetuates a trifling discharge from it. Hence it is impossible to judge whether or not the sinus has closed, except by examining it from time to time with a probe, which should be dipped in the antiseptic oil, and passed in between folds of the antiseptic rag. This may seem a refinement, but if we could see with the naked eye a few only of the septic organisms that people every cubic inch of the atmosphere of an hospital ward, we should rather wonder that the antiseptic treatment is ever successful than omit any precautions in conducting it.

The putty used in treating abscesses has proved very valuable in simplifying the treatment of compound fracture, and enlarging the range of its applicability, and also in dealing with incised wounds on the antiseptic principle. But I must defer a notice of these matters to a future occasion.—*Lancet, July* 27, 1867, *p.* 95.

40.—ON THE ANTISEPTIC PRINCIPLE IN THE PRACTICE OF SURGERY.

By Joseph Lister, Esq., F.R.S., Professor of Surgery in the University of Glasgow.

[The following paper by Prof. Lister was read before the British Medical Association, in Dublin, on the 9th of August, 1867.]

In the course of an extended investigation into the nature of inflammation, and the healthy and morbid conditions of the blood in relation to it, I arrived, several years ago, at the conclusion that the essential cause of suppuration in wounds is decomposition, brought about by the influence of the atmosphere upon blood or serum retained within them, and, in the case of contused wounds, upon portions of tissue destroyed by the violence of the injury.

To prevent the occurrence of suppuration, with all its attendant risks, was an object manifestly desirable; but till lately apparently unattainable, since it seemed hopeless to attempt to exclude the oxygen, which was universally regarded as the agent by which putrefaction was effected. But when it had been shown by the researches of Pasteur that the septic property of the atmosphere depended, not on the oxygen or any gaseous constituent, but on minute organisms suspended in it, which owed their energy to their vitality, it occurred to me that decomposition in the injured part might be avoided without excluding the air, by applying as a dressing some material capable of destroying the life of the floating particles.

Upon this principle I have based a practice of which I will now attempt to give a short account.

The material which I have employed is carbolic or phenic acid, a volatile organic compound which appears to exercise a peculiarly destructive influence upon low forms of life, and hence is the most powerful antiseptic with which we are at present acquainted.

The first class of cases to which I applied it was that of compound fractures, in which the effects of decomposition in the injured part were especially striking and pernicious. The results have been such as to establish conclusively the great principle, that *all the local inflammatory mischief and general febrile disturbance which follow severe injuries are due to the irritating and poisoning influence of decomposing blood or sloughs.* For these evils are entirely avoided by the antiseptic treatment, so that limbs which otherwise would be unhesitatingly condemned to amputation may be retained with confidence of the best results.

In conducting the treatment, the first object must be the destruction of any septic germs which may have been introduced into the wound, either at the moment of the accident or during the time which has since elapsed. This is done by introducing the acid of full strength into all accessible recesses of the wound by means of a piece of rag held in dressing forceps and dipped in the liquid.* This I did not venture to do in the earlier cases; but experience has shown that the compound which carbolic acid forms with the blood, and also any portions of tissue killed by its caustic action, including even parts of the bone, are disposed of by absorption and organisation, provided they are afterwards kept from decomposing. We are thus enabled to employ the antiseptic treatment efficiently at a period after the occurrence of the injury at which it would otherwise probably fail. Thus I have now under my care in the Glasgow Infirmary a boy who was admitted with compound fracture of the leg as late as eight and a half hours after the accident, in whom nevertheless all local and constitutional disturbance was avoided by means of carbolic acid, and the bones were firmly united five weeks after his admission.

The next object to be kept in view is to guard effectually against the spreading of decomposition into the wound along the stream of blood and serum which oozes out during the first few days after the accident, when the acid originally applied has been washed out, or dissipated by absorption and evaporation. This part of the treatment has been greatly improved during the last few weeks. The method which I have hitherto published consisted in the application of a piece of lint dipped in the acid, overlapping the sound skin to some extent, and covered with a tin cap, which was daily raised in order to touch the surface of the lint with the antiseptic. This method certainly succeeded well with wounds of moderate size; and, indeed, I may say that in all the many cases of this kind which have been so treated by myself or my house-surgeons, not a single failure has occurred, When, however, the wound is very large, the flow of blood and serum is so profuse, especially during the first twenty-four hours, that the antiseptic application cannot prevent the spread of decomposition into the interior unless it overlaps the sound skin for a very considerable distance, and this was inadmissible by the method described above, on account of the extensive sloughing of the surface of the cutis which it would involve. This difficulty has, however, been overcome by employing a paste composed of common whitening (carbonate of lime) mixed with a solution of one part of carbolic acid in four parts of boiled linseed oil, so as to form a firm putty. This application contains the acid in too dilute a form to excoriate the skin, which it may be made to cover to any extent that may be thought desirable, while its substance serves as a reservoir of the antiseptic material. So long as any discharge continues, the paste should be changed daily; and, in order to prevent the chance of mischief occurring during the process, a piece of rag dipped in the solution of carbolic acid in oil is put on next the skin, and maintained there permanently, care being taken to avoid raising it along with the putty. This rag is always kept in an antiseptic condition from contact with the paste above it, and destroys any germs that may fall upon it during the short time that should alone be allowed to pass in the changing of the dressing. The putty should be in a layer about a quarter of an inch thick, and may be advantageously applied rolled out between two pieces of thin calico, which maintain it in the form of a continuous sheet, that may be wrapped in a moment round the whole circumference of a limb, if this be thought desirable, while the putty is prevented by the calico from sticking to the rag which is next the skin.* When all discharge has ceased, the use of the paste is discontinued, but the original rag is left adhering to the skin till healing by scabbing is supposed to be complete. I have at present in the hospital a man with severe compound fracture of both bones of the left leg, caused by direct violence, who, after the cessation of the sanious discharge under the use of the paste, without a drop of pus appearing, has been treated for the last two weeks exactly as if the fracture were a simple one. During this time the rag, adhering by means of a crust of inspissated blood collected beneath it, has continued perfectly dry, and it will be left untouched till the usual period for removing the splints in a simple fracture, when we may fairly expect to find a sound cicatrix beneath it.

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* The addition of a few drops of water to a considerable quantity of the crystallised acid induces it to assume permanently the liquid form.

We cannot, however, always calculate on so perfect a result as this. More or less pus may appear after the lapse of the first week; and the larger the wound the more likely is this to happen. And here I would desire earnestly to enforce the necessity of persevering with the antiseptic application, in spite of the appearance of suppuration, so long as other symptoms are favourable. The surgeon is extremely apt to suppose that any suppuration is an indication that the antiseptic treatment has failed, and that poulticing or water-dressing should be resorted to. But such a course would in many cases sacrifice a limb or a life. I cannot, however, expect my professional brethren to follow my advice blindly in such a matter, and therefore I feel it necessary to place before them, as shortly as I can, some pathological principles, intimately connected not only with the point we are immediately considering, but with the whole subject of this paper.

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* In order to prevent evaporation of the acid, which passes readily through any organic tissue, such as oiled silk or gutta percha, it is well to cover the paste with a sheet of block tin, or tinfoil strengthened with adhesive plaster. The thin sheet-lead used for lining tea-chests will also answer the purpose, and may be obtained from any wholesale grocer.

If a perfectly healthy granulating sore be well washed and covered with a plate of clean metal, such as block tin, fitting its surface pretty accurately, and overlapping the surrounding skin an inch or so in every direction, and retained in position by adhesive plaster and a bandage, it will be found, on removing it after twenty-four or forty-eight hours, that little or nothing that be called pus is present, merely a little transparent fluid, while at the same time there is an entire absence of the unpleasant odour invariably perceived when water-dressing is changed. Here the clean metallic surface presenting no recesses, like those of porous lint, for the septic germs to develop in, the fluid exuding from the surface of the granulations has flowed away undecomposed, and the result is absence of suppuration. This simple experiment illustrates the important fact, that granulations have no inherent tendency to form pus, but do so only when subjected to a preternatural stimulus. Further, it shows that the mere contact of a foreign body does not of itself stimulate granulations to suppurate; whereas the presence of decomposing organic matter does. These truths are even more strikingly exemplified by the fact, which I have elsewhere recorded, that a piece of dead bone, free from decomposition, may not only fail to induce the granulations round it to suppurate, but may actually be absorbed by them; whereas a bit of dead bone soaked with putrid pus infallibly induces suppuration in its vicinity.

Another instructive experiment is to dress a granulating sore with some of the putty above described, overlapping the sound skin extensively, when we find in the course of twenty-four hours that pus has been produced by the sore, although the application has been perfectly antiseptic; and, indeed, the larger the amount of carbolic acid in the paste the greater is the quantity of pus formed, provided we avoid such a proportion as would act as a caustic. The carbolic acid, though it prevents decomposition, induces suppuration—obviously by acting as a chemical stimulus; and we may safely infer that putrescent organic materials (which we know to be chemically acrid) operate in the same way.

In so far, then, carbolic acid and decomposing substances are alike—namely, that they induce suppuration by chemical stimulation, as distinguished from what may be termed simple inflammatory suppuration, such as that in which ordinary abscesses originate, where the pus appears to be formed in consequence of an excited action of the nerves, independently of any other stimulus. There is, however, this enormous difference between the effects of carbolic acid and those of decomposition—viz., that carbolic acid stimulates only the surface to which it is first applied, and every drop of discharge that forms weakens the stimulant by diluting it. But decomposition is a self-propagating and self-aggravating poison ; and if it occurs at the surface of a severely injured limb, it will spread into all its recesses so far as any extravasated blood or shreds of dead tissue may extend, and, lying in these recesses it will become from hour to hour more acrid till it acquires the energy of a caustic, sufficient to destroy the vitality of any tissues naturally weak from inferior vascular supply, or weakened by the injury they sustained in the accident.

Hence, it is easy to understand how, when a wound is very large, the crust beneath the rag may prove here and there insufficient to protect the raw surface from the stimulating influence of the carbolic acid in the putty, and the result will be, first, the conversion of the tissues so acted on into granulations, and subsequently the formation of more or less pus. This, however, will be merely superficial, and will not interfere with the absorption and organisation of extravasated blood or dead tissues in the interior ; but, on the other hand, should decomposition set in before the internal parts have become securely consolidated, the most disastrous results may ensue.

I left behind me in Glasgow a boy, 13 years of age, who between three and four weeks previously met with a most severe injury to the left arm, which he got entangled in a machine at a fair. There was a wound six inches long and three inches broad, and the skin was very extensively undermined beyond its limits, while the soft parts generally were so much lacerated that a pair of dressing forceps introduced at the wound, and pushed directly inwards, appeared beneath the skin at the opposite aspect of the limb. From this wound several tags of muscle were hanging, and among them there was one consisting of about three inches of the triceps in almost its entire thickness ; while the lower fragment of the bone, which was broken high up, was protruding four and a half inches, stripped of muscle, the skin being tucked in under it. Without the assistance of the antiseptic treatment, I should certainly have thought of nothing else but amputation at the shoulder-joint ; but as the radial pulse could be felt, and the fingers had sensation, I did not hesitate to try to save the limb, and adopted the plan of treatment above described, wrapping the arm from the shoulder to below the elbow in the antiseptic application, the whole interior of the wound, together with the protruding bone, having previously been freely treated with strong carbolic acid. About the tenth day the discharge, which up to that time had been only sanious and serous, showed a slight admixture of slimy pus, and this increased till, a few days before I left, it amounted to about three drachms in twenty-four hours. But the boy continued, as he had been after the second day, free from unfavourable symptoms, with pulse, tongue, appetite, and sleep natural, and strength increasing, while the limb remained, as it had been from the first, free from swelling, redness, or pain. I therefore persevered with the antiseptic dressing, and before I left, the discharge was already somewhat less, while the bone was becoming firm. I think it likely that in that boy's case I should have found merely a superficial sore had I taken off all the dressings at the end of three weeks, though, considering the extent of the injury, I thought it prudent to let the month expire before disturbing the rag next the skin. But I feel sure that if I had resorted to ordinary dressing when the pus first appeared, the progress of the case would have been exceedingly different.

The next class of cases to which I have applied the antiseptic treatment is that of abscesses. Here, also, the results have been extremely satisfactory, and in beautiful harmony with the pathological principles indicated above. The pyogenic membrane, like the granulations of a sore, which it resembles in nature, forms pus, not from any inherent disposition to do so, but only because it is subjected to some preternatural stimulation. In an ordinary abscess, whether acute or chronic, before it is opened, the stimulus which maintains the suppuration is derived from the presence of the pus pent up within the cavity. When a free opening is made in the ordinary way, this stimulus is got rid of; but the atmosphere gaining access to the contents, the potent stimulus of decomposition comes into operation, and pus is generated in greater abundance than before. But when the evacuation is effected on the antiseptic principle, the pyogenic membrane, freed from the influence of the former stimulus without the substitution of a new one, ceases to suppurate (like the granulations of a sore under metallic dressing), furnishing merely a trifling amount of clear serum, and, whether the opening be dependent or not, rapidly contracts and coalesces. At the same time any constitutional symptoms previously occasioned by the accumulation of the matter are got rid of without the slightest risk of the irritative fever or hectic hitherto so justly dreaded in dealing with large abscesses.

In order that the treatment may be satisfactory, the abscess must be seen before it has opened. Then, except in very rare and peculiar cases, there are no septic organisms in the contents, so that it is needless to introduce carbolic acid into the interior. Indeed, such a proceeding would be objectionable, as it would stimulate the pyogenic membrane to unnecessary suppuration. All that is necessary is to guard against the introduction of living atmospheric germs from without, at the same time that free opportunity is afforded for the escape of discharge from within.

I have so lately given elsewhere a detailed account of the method by which this is effected, that it is needless for me to enter into it at present, further than to say that the means employed are the same as those described above for the superficial dressing of compound fractures—namely, a piece of rag dipped in the solution of carbolic acid in oil, to serve as an antiseptic curtain, under cover of which the abscess is evacuated by free incision; and the antiseptic paste, to guard against decomposition occurring in the stream of pus that flows out beneath it: the dressing being changed daily till the sinus has closed.

The most remarkable results of this practice in a pathological point of view have been afforded by cases where the formation of pus depended upon disease of bone. Here the abscesses, instead of forming exceptions to the general class in the obstinacy of the suppuration, have resembled the rest in yielding in a few days only a trifling discharge; and frequently the production of pus has ceased from the moment of the evacuation of the original contents. Hence it appears that caries, when no longer labouring, as heretofore, under the irritation of decomposing matter, ceases to be an opprobrium of surgery, and recovers like other inflammatory affections. In the publication before alluded to I have mentioned the case of a middle-aged man with psoas abscess depending on diseased bone, in whom the sinus finally closed after months of patient perseverance with the antiseptic treatment. Since that article was written I have had another instance of success, equally gratifying, but differing in the circumstance that the disease and the recovery were both more rapid in their course. The patient was a blacksmith who had suffered four and a half months before I saw him from symptoms of ulceration of cartilage in the left elbow. These had latterly increased in severity, so as to deprive him entirely of his night's rest and of appetite. I found the region of the elbow greatly swollen, and on careful examination discovered a fluctuating point at the outer aspect of the articulation. I opened it on the antiseptic principle, the incision evidently penetrating to the joint, giving exit to a few drachms of pus. The medical gentleman under whose care he was (Dr. Macgregor of Glasgow) supervised the daily dressing with the carbolic-aid paste till the patient went to spend two or three weeks at the coast, when his wife was entrusted with it. Just two months after I opened the abscess he called to show me the limb, stating that the discharge had for at least two weeks been as little as it then was—a trifling moisture upon the paste, such as might be accounted for by the little sore caused by the incision. On applying a probe guarded with an antiseptic rag, I found that the sinus was soundly closed, while the limb was free from swelling or tenderness ; and, although he had not attempted to exercise it much, the joint could already be moved through a considerable angle. Here the antiseptic principle had effected the restoration of a joint which on any other known system of treatment must have been excised.

Ordinary contused wounds are of course amenable to the same treatment as compound fractures, which are a complicated variety of them. I will content myself with mentioning a single instance of this class of cases. In April last a volunteer was discharging a rifle, when it burst, and blew back the thumb with its metacarpal bone, so that it could be bent back as on a hinge at the trapezial joint, which had evidently been opened, while all the softs parts between the metacarpal bones of the thumb and forefinger were torn through. I need not insist before my present audience on the ugly character of such an injury. My house-surgeon, Mr. Hector Cameron, applied carbolic acid to the whole raw surface, and completed the dressing as if for compound fracture. The hand remained free from pain, redness, or swelling, and with the exception of a shallow groove, all the wound consolidated without a drop of matter, so that if it had been a clean cut, it would have been regarded as a good example of primary union. The small granulating surface soon healed, and at present a linear cicatrix alone tells of the injury he had sustained, while his thumb has all its movements and his hand a firm grasp.

If the severest forms of contused and lacerated wounds heal thus kindly under the antiseptic treatment, it is obvious that its application to simple incised wounds must be merely a matter of detail. I have devoted a good deal of attention to this class, but I have not as yet pleased myself altogether with any of the methods I have employed. I am, however, prepared to go so far as to say that a solution of carbolic acid in twenty parts of water, while a mild and cleanly application, may be relied on for destroying any septic germs that may fall upon the wound during the performance of an operation; and also that for preventing the subsequent introduction of others, the paste above described, applied as for compound fractures, gives excellent results. Thus I have had a case of strangulated inguinal hernia, in which it was necessary to take away half a pound of thickened omentum, heal without any deep-seated suppuration or any tenderness of the sac or any fever; and amputations, including one immediately below the knee, have remained absolutely free from constitutional symptoms.

Further, I have found that when the antiseptic treatment is efficiently conducted, ligatures may be safely cut short and left to be disposed of by absorption or otherwise. Should this particular branch of the subject yield all that it promises, should it turn out on further trial that when the knot is applied on the antiseptic principle, we may calculate as securely as if it were absent on the occurrence of healing without any deep-seated suppuration; the deligation of main arteries in their continuity will be deprived of the two dangers that now attend it—viz., those of secondary hemorrhage and an unhealthy state of the wound. Further, it seems not unlikely that the present objection to tying an artery in the immediate vicinity of a large branch may be done away with; and that even the innominate, which has lately been the subject of an ingenious experiment by one of the Dublin surgeons on account of its well known fatality under the ligature from secondary hemorrhage, may cease to have this unhappy character, when the tissues in the vicinity of the thread, instead of becoming softened through the influence of an irritating decomposing substance, are left at liberty to consolidate firmly near an unoffending though foreign body.

It would carry me far beyond the limited time which, by the rules of the Association, is alone at my disposal, were I to enter into the various applications of the antiseptic principle in the several special departments of surgery.

There is, however, one point more that I cannot but advert to—namely, the influence of this mode of treatment upon the general healthiness of an hospital. Previously to its introduction, the two large wards in which most of my cases of accident and of operation are treated were amongst the unhealthiest in the whole surgical division of the Glasgow Royal Infirmary, in consequence, apparently, of those wards being unfavourably placed with reference to the supply of fresh air; and I have felt ashamed, when recording the results of my practice, to have so often to allude to hospital gangrene or pyæmia. It was interesting, though melancholy, to observe that, whenever all, or nearly all, the beds contained cases with open sores these grevious complications were pretty sure to show themselves; so that I came to welcome simple fractures, though in themselves of little interest either for myself or the students, because their presence diminished the proportion of open sores among the patients. But since the antiseptic treatment has been brought into full operation, and wounds and abscesses no longer poison the atmosphere with putrid exhalations, my wards, though in other respects under precisely the same circumstances as before, have completely changed their character; so that during the last nine months not a single instance of pyæmia, hospital gangrene, or erysipelas has occurred in them.

As there appears to be no doubt regarding the cause of this change, the importance of the fact can hardly be exaggerated.—*Lancet, Sept.* 21, 1867, *p.* 353.

41.—ON THE USE OF CARBOLIC ACID IN WOUNDS, COMPOUND FRACTURES, &c.

By Dr. J. R. Wolfe, Aberdeen, and Sir J. Y. Simpson, Edinburgh.

[The merit has been given to Prof. Lister, of Glasgow, of having been the first to apply carbolic acid to wounds, compound fractures, &c. Dr. Wolfe, however, states that he had experimented on the use of carbolic acid both externally and internally since the summer of 1864, as will be seen from the following passage from his paper published in the *Med. Times and Gazette* of Nov. 25, 1865, which will be found in *Retrospect*, Vol. 53, p. 108.]

"The only objection to the use of carbolic acid is its strong and rather disagreeable smell. To obviate this I have made some experiments, and find that cotton wool saturated in the pure acid, and then pressed to get rid of the excess of acid, and dried, leaves enough of the acid to act upon the wound without leaving so much as to make the smell disagreeable. The cotton so prepared is kept in a tin pot to prevent volatilisation of the acid. As far as my experiments have gone I am satisfied with its beneficial results. It causes a healthy granulating surface, counteracting the tendency to hyperplasia and suppuration. But my study of the subject is not sufficiently advanced to enable me to enter into particulars in this communication. I may express it, however, as my opinion, that its employment for dressing wounds will greatly lessen the risk of purulent and erysipelatous infection in our surgical wards."

It was, therefore, with the highest satisfaction that I read those interesting papers by Prof. Lister in the Lancet of the current year, referred to in your leading article of last week; knowing that, with the vast materials at his disposal in the Glasgow Infirmary, that eminent surgeon would be able to do ample justice to that important subject.

Yet I must add that my belief in the efficacy of carbolic acid is not founded upon the theory of M. Pasteur; for after the counter experiments by MM. Joly and Pouchet, the *panspermatism* hypothesis rests, in my opinion, upon debatable ground. I am rather inclined to attribute the action of carbolic acid entirely to its antiseptic properties—that is, of preserving organisable matter from passing into a putrescent condition. Indeed, the remarkable cases of Professor Paget, quoted in your article referred to, seem to militate against the idea of basing its beneficial action upon M. Pasteur's doctrine; for, seeing that "the best results in case of pyæmia were in patients that had been kept day and night in a current of wind," this tends to show that it is not the atmosphere, fraught with sperms or sporules, which acts deleteriously, but rather the contaminated *hospital air* which commits such havoc upon wounds, against which the carbolic acid is a valuable protector and modificator.—*Lancet, Sept.* 28, 1867, *p.* 410.

[Sir JAMES SIMPSON also disputes the originality of Prof. Lister's views on this subject, and he gives good reasons for what he says. Prof. Lister's papers have all been published in 1867, but in 1863 Dr. Lemaire, of Paris, published a work on the same subject. Sir James says :]

Originally in France carbolic acid was employed in surgery in the form of a powder made of coal-tar and lime, as suggested by MM. Corne and Demeaux in 1859. Shortly after its introduction surgical experiments were made with it in the hospitals of Paris and in the French army in Italy after the battles of Magenta and Solferino. These experiments gave it, to use the language of Dr. Lemaire, "un grand retentissement." "Le fait principal," he adds, "la désinfection, était admis et vanté avec enthousiasme." M. Cabannes subsequently suggested the mixture of coal-tar with other mineral and various vegetable powders, besides gypsum ; but their introduction was, as stated by Lemair, difficult when the wounds were anfractuous or fistulous. Afterwards, M. Le Boeuf made a still more elegant and useful preparation of the coal-tar, in the form of a fixed emulsion with saponine ; a form which Dr. Lemaire seems still to prefer, in some surgical complications, to the pure carbolic acid diluted or otherwise modified.

At last Mr. Calvert, in England, and Parisel and Bouchardat in France, showed by their chemical investigations that carbolic acid is the essential principle to which coal-tar owes its antiseptic properties, as opium owes its soporific properties to morphia, cinchona its febrifuge properties to quinine, &c.

Mr. Lister remarks (Lancet, Sept 21st, 1867) that when it had been shown, by the researches of Pasteur, that the septic property of the atmosphere depended on minute living organisms suspended in it, "it occurred to me," to use his own words, "that decomposition in the injured part might be avoided, without excluding the air, by applying as a dressing some material capable of destroying the life of the floating particles. Upon this principle I have based a practice. . . .

The material which I have employed is carbolic or phenic acid, a volatile organic compound, which appears to exercise a peculiarly destructive influence upon low forms of life."

Now the very same theory, the very same practice, and the very same means of reducing that theory to practice, have all been worked out and published on the continent years ago by Lemaire, Déclat, Küchenmeister, and others. The strongest averments cannot alter the simplest dates, and I fear that it will not redound to the credit of English surgery to claim what most certainly does not pertain to it.

In his work on Carbolic Acid, Dr. Lemaire enters elaborately and philosophically into the question of septic infusoria in the atmosphere, and discusses the opinions of Schultze, Schwann, Pasteur, Pouchet, Helmholtz, Milne-Edwards, Bernard, Gratiolet, and others, upon the subject. His conclusions are that the air does contain these alleged low vital organisms; that when admitted to wounds, ulcers, &c., they produce a decomposition in the part and secreted fluids, aiding the formation of pus; that this decomposition is effected by a vital action similar to the production and multiplication of organisms in the process of fermentation; and that carbolic acid—even in very small doses—has the power of preventing and of arresting any such decomposing effects from these organisms by at once and immediately destroying the life of the organisms themselves.

Dr. Lemaire has applied carbolic acid to the treatment of numerous *Other Surgical Complications* and diseases besides the three especially mentioned by Mr. Lister. In his work he has adduced, for example, cases and remarks to show its utility as an antiseptic and healing agent in simple scrofulous ulcers, &c.; in burns; in venomous bites; in dissection wounds; in sloughing and gangrene of the soft tissues, (the ravages of gangrene, he says, have often thus been stopped as if by enchantment;) in inflammation, caries, and necrosis of bone; in inflammation and abscess of joints; in whitlows; in carbuncles, (where its action, he avers, is marvellous, probably from its containing living organisms, which he thinks he has detected with the microscope;) in lupus; in cancerous discharges; in ozæna and otorrhœa; in chancre; in gonorrhœa; in catarrh of the bladder as an injection, and as an injection also in fistulæ, and into the cavities of cysts, &c. He gives various disquisitions, with cases illustrative of its employment in different forms of skin diseases particularly in those which arise from the presence and irritation of low animal and vegetable organisms. It aborts pustules of cow-pox and small-pox when applied to them. Internally, again, he has used it in the form of vapour, solution, &c., to aphthæ, angina, diphtheria, croup, whooping-cough, &c., and in some diseases of the intestinal canal, as dyspepsia, (would not the form complicated with sarcinæ be benefited by it as much as by sulphurous acid compounds ?), in dysentery, cholera, &c. And he suggests further trial of it internally in some contagious diseases, as scarlatina and measles; in agues and malarious diseases, and various other maladies, where low vegetable or animal organisms may be possibly supposed to exist. This rapid and imperfect enumeration shows in itself the great thought and attention which Dr. Lemaire has bestowed on the therapeutical action and applications of carbolic acid; in that respect, as well as in originality, leaving Prof. Lister very far behind him. On one or two points—but these of a most doubtful character—Prof. Lister goes, perhaps, further, for he seems actually to believe that the use of carbolic acid may lead to the absorption of a piece of necrosed bone, or of a silken ligature when cut short and left in a wound—a kind of surgical belief in which I think he will get few or no disciples to join him at the present day, though such physiological deeds were credited by some of the olden surgical pathologists.

They seem to have used carbolic acid, therapeutically and surgically, earlier in Germany than in France. In the Year-Book of the Sydenham Society for 1860, at p. 486, we find a notice of the disinfection of the sewage of Carlisle by carbolic acid, and brief analyses of Demeaux's disinfecting charpie and of Corne and Demeaux's disinfecting powder; and at p. 487 a notice of the disinfectant powers of carbolic acid itself, by Dr. Küchenmeister of Dresden. "Under the name of spirol," it is said, "Küchenmeister describes phenic or carbolic acid as a colourless crystalline body, which fuses at 34° (cent.), and boils at 187°; and is obtained from oil of coal tar, as well as by the distillation of salicine with lime. Küchenmeister has applied the carbolic acid with the most satisfactory results both in medical and surgical practice, and as a means of arresting putrefaction and *preventing the development of fungi.*" This last remark is the more interesting as it preceded the observations of Pasteur.

One great and most laudable object which Professor Lister evidently has in view in using carbolic acid as a local dressing to wounds is to close these wounds entirely by the first intention, and without any suppuration; so far, by this means, averting the mischances of surgical fever. But these paramount objects have been attained in the hospital of Aberdeen by the use of acupressure. In the Surgical Section of the Association to which Mr. Lister read his paper in Dublin, Dr. Pirrie, of Aberdeen, stated that, for example, out of twelve cases of excision of mammary tumours treated with acupressure in his practice during the last two or three years, eight out of the resulting wounds had closed entirely by the first intention, and without the formation of a single drop of pus.—*Lancet, Nov.* 2, 1867, *p.* 546.

[Nevertheless we think that great credit is due to Professor Lister for so prominently bringing this treatment before the profession, and although there seems to be "nothing new under the sun," it is almost as creditable to bring an old thing into practice as to originate it at first.]

42.—THE EMPLOYMENT OF CHLORIDE OF ZINC AND CARBOLIC ACID IN WOUNDS.

Case under the care of John Wood, Esq., at King's College Hospital.

[During the two or three months immediately preceding the publication of the following short article, carbolic acid and chloride of zinc have been extensively used by the assistant-surgeons at King's College Hospital. The former is used to suppurating wounds, and the latter to the parts involved in formidable operations, (see article by Mr. De Morgan, *Retrospect*, Vol. 53, p. 147.)

On Aug. 31st a man was admitted into the hospital whose hand had just been severely injured by the explosion of a box of detonating powder. The wrist and palm were very much lacerated, two of the metacarpal bones were comminuted, and the skin and tendons were stripped off from the phalanges of the fingers. The hand was amputated at the wrist-joint, and a thin slice removed from the ends of the radius and ulna. Before the flaps were brought together, a solution of chloride of zinc, thirty grains to the ounce, was applied to every part. Although the incisions were made in close proximity to the seat of injury, and damaged tissue was included in the flaps, the wound rapidly healed. There was very little suppuration, and on Sept. 4th the man was discharged from the hospital. On Sept. 12th the wound was completely healed, and the man presented a sound, firm, and painless stump.

On Sept. 8th a large and rapidly growing tumour was removed from the gluteal region of a man, 25 years of age. This growth was of eighteen months' duration, larger than the adult head, and evidently of a malignant character. Two days before the operation the man had a febrile attack, the tumour became painful, and the skin covering it inflamed and glossy. When the growth was exposed by extensive skin incisions, its upper and superficial part was found to have degenerated into a black, semifluid mass, of an intolerably fetid odour; this was scooped out by the hand, and the deep-seated parts, which were white, firm, and marked with all the external characteristics of encephaloma, were carefully dissected away from the recesses of the gluteal region. The sacro-sciatic ligament were exposed, and round this a lobe of the tumour passed into the interior of the pelvis. The black and unhealthy-looking muscular tissue forming the thickness of the flaps was then swabbed with the solution of chloride of zinc. The man has progressed favourably since the operation, and without a single bad symptom. A drainage-tube was passed through the wound shortly after, but, notwithstanding the presence of this, the edges of the incision quickly united, and the wound is now healing with remarkable rapidity considering its depth and extent, and without any discharge of pus. Carbolic acid has been applied superficially, and the drainage-tube was passed in order to prevent putrid and unhealthy fluids from collecting in the deep parts of the gluteal region.—*Lancet, Oct.* 12, 1867, *p.* 455.

43.—SULPHUROUS ACID AS APPLIED TO WOUNDS AND SORES.

By Dr. James Dewar, Kirkcaldy.

It is obvious that any means of promoting the speedy union of parts which have been divided during surgical operations would materially contribute to the patient's advantage, and lessen the risks to which he is otherwise exposed. The following case, as showing that the existence of pus is not only an unnecessary but a preventible accident, has an important bearing upon the subject of surgical fever, and would encourage the hope that even over it we may be able to exercise some control by anticipating the evils associated with its existence. I have long been impressed with the fact of there being an antagonism between sulphurous acid and pus, but have not till now had an opportunity of fully testing its value. Miss —, a young woman with a tumour in the breast; she had chloroform, and the tumour, which was about the size of a half-closed fist, was removed by a wound of about six inches long. There was little bleeding, and there was no occasion for ligature. The raw surface was carefully sponged with sulphurous acid, and the edges adjusted by four silver sutures. A piece of lint soaked in the liquid was laid over the wound, and this covered with gutta-percha, the dressing being changed every six hours. From that time till now the patient has never had the slightest uneasiness in the wound, which she could bear to have handled without apprehension; indeed, she said that if she did not see the wound she would not be aware of its existence. Union was complete within twelve hours. The stitches were removed on the third day, and two days thereafter my patient was in the garden. She assures me that there never was a stain upon the dressing.

A young man had his hand severely cut when working at a circular saw. The wound was closed in the usual way, and the acid applied, as in the preceding case. The pain instantly ceased, and the edges have since united without any appearance of pus.

I may mention that some months ago Professor Syme had a case in which the result was very similar. He told me that the sulphurous acid spray was applied to the raw surface, and that when he went to dress it for the first time he was delighted to find the healing process in an unusual state of forwardness.

I hope that some of your readers will be induced to give this mode of treatment a trial, and have every confidence that their experience of it will be satisfactory. Sulphurous acid is superior in efficiency to carbolic acid, and is entirely free from the objection applicable to the latter—viz., of being a powerful irritant and having a disgusting smell.—*Medical Times and Gazette, Sept.* 21, 1867, *p.* 318.

44.—ON THE TREATMENT OF INCISED WOUNDS WITH A VIEW TO UNION BY THE FIRST INTENTION.

By James Syme, Esq., F.R.S.E., Surgeon to the Queen in Scotland.

[Observations lately made as to the effect of atmospheric atoms in causing decomposition, has led to a complete revolution of ideas on the subject of the treatment of wounds and abscesses. The observations were originally made by M. Pasteur, and to Prof. Lister of Glasgow, is due the credit of first applying them in the surgical treatment of disease.]

In cases of bruise, fracture, dislocation, and even operations of tenotomy, large quantities of blood are frequently effused more or less deeply under the integuments without causing any bad effect, and quickly disappear by means of absorption. How, then, does it happen that blood collected in the cavity of a wound should be productive of so much mischief? It can only do so, as Mr. Lister has shown, through the decomposing influence of atmospheric air, loaded with its myriads of organic atoms, and, therefore, if protected from this agency, would be no more hurtful than in the circumstances just mentioned. He has accordingly found, as stated in the preceding numbers of this journal, that wounds of the most formidable character may be divested of all their alarming features by means of carbolic acid, applied so as to prevent the impure air from entering.

This remarkable fact has led me to consider the expediency of resorting more frequently than heretofore to the use of "torsion" for the suppression of hemorrhage. Ever since this method was proposed by the late M. Amussat I have employed it in wounds of the check and lips, and also occasionally in more serious operations, to let my pupils see that it was quite effectual, but have been prevented from adopting it for general use, by fearing that the absence of ligatures, which serve as conductors of the discharge, would increase the risk of blood or serum being pent up in the cavity. Feeling now happily relieved from any apprehension of evil being thus produced, I think "torsion" may in many, if not in all cases be employed with advantage, instead of the ligature. In order to perform the process effectually, it is necessary that the artery should be seized by catch-forceps, and twisted until they become loose. It has been alleged that such a liberty with the vessel must cause it to slough, and thus disturb the adhesive action. But as this objection is altogether theoretical and contradicted by experience, it is unworthy of notice.

In illustration of what has been said, the two following cases may be mentioned as sufficient to substantiate the facts—

1st That torsion effectually restrains the hemorrhage of ordinary-sized arteries.

2nd. That its action upon them does not prevent union by the first intention.

3rd. That protection from the air prevents decomposition of the blood.

Case 1.—On the 27th of May I amputated the middle finger of a young woman in the hospital at the metacarpal joint for disease of three years' standing, and arrested the bleeding by torsion of the arteries, which were three in number. A mixture of carbolic acid and linseed oil, in the proportion of one to five, having then been freely applied to the surface of the wound, I tied the adjoining fingers together so as to retain the cut edges in contact, and placed over them a piece of lint soaked in the carbolic mixture. The result at the end of three days was complete and perfect union without any discharge of matter.

Case 2.—On the 11th of June Mr. Annandale removed a tumour from the chest of a male patient by an incision ten inches in length, and employed the torsion for five arteries, two of which were of considerable size. He then applied the carbolic mixture freely to the wound, brought its edges together by silver sutures, and covered them with lint soaked in the protective fluid. Three days afterwards I saw the wound soundly healed without a drop of matter.

The following case is sufficient to prove—if proof were wanting—that ligatures do not prevent primary union beyond the extent which they occupy.

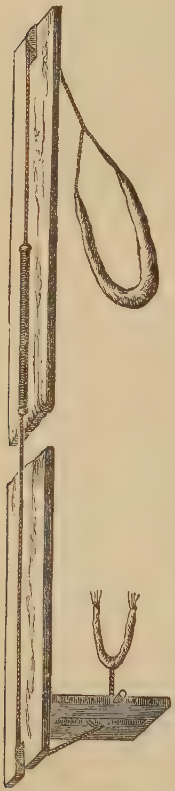
Case 3.—On the 11th of June a young man was admitted into the hospital on account of a large brachial aneurism, the size of an orange, at the bend of his elbow, which had resulted from the thrust of a file two months before the time of application. I laid open the sac to its full extent, removed the blood and coagulum, tied the humeral artery on both sides of the orifice, twisted a small vessel that bled, rinsed out the cavity with the carbolic mixture, and applied a piece of lint soaked in it on the wound. There did not follow the slightest local or constitutional disturbance; and complete adhesion took place, except at the small point where the ligatures lay, from which a few drops of pus were discharged. On the fifth day one of the ligatures came out, and on the sixth the other; so that on the seventh day the recovery might be regarded as perfect.—*Lancet, July* 6, 1867, *p.* 5.

45.—ON THE TREATMENT OF FRACTURES BY ELASTIC EXTENSION.

By H. Worthington, Esq., Late House-Surgeon to the Middlesex Hospital.

The many cases of fracture of the lower extremities admitted into this hospital, and the labour and pain necessitated in their reduction and successful treatment, induced the trial of elastic extension. Its mode of application is easy, and the results, as far as they have gone, have proved highly satisfactory. The following is a description of the way in which elastic extension is applied at the Middlesex Hospital.

The splint employed is of suitable size, and a modification of that recommended by Mr. Barwell for extending the leg in hip-joint disease. It consists of a long, straight, stout splint, with a piece of wood about a foot in length let in at right angles to it about six inches from its lower end. The long arm of the splint is well padded on its inner side. An inch or two from the upper end of the splint is a small pulley, fixed in an aperture made for the purpose; a similar one is placed at the lower end. That part of the splint which is let in at right angles, is provided with a slit extending nearly its whole length, admitting another pulley of similar size, which can be shifted to various parts by means of an iron pin passing through holes bored in the sides of the cross-piece. A strong piece of strapping is now applied to the sides of the leg, leaving a loop at the sole of the foot as in the ordinary way, care being taken that the loop shall be large enough to admit a tightly rolled leg-bandage; this being placed transversely at about the middle of the sole of the foot, so that it is enabled to receive all lateral pressure when extension is applied. Without this, the strapping would cut the hollow and outer side of the foot, and give rise to considerable pain and inconvenience. In cases of fracture of the leg, the strapping should not come above the seat of injury. The foot should now be bandaged, so as to retain the strapping firmly. A piece of strong cord is then fastened to the loop of plaster, so as to include it and the roller, and this gives us a fixed point to pull upon from below. The cord, thus fastened, is next passed over the pulley which lies in the slit in the cross-piece, then over the pulley at the lower end of the splint, and is carried to about the middle of the outside of the long side-piece. Here it is attached to one end of a patent elastic door-spring, or accumulator. To the upper end of the elastic is attached a piece of wood, about four inches long, one inch broad, and half an inch thick, perforated by three holes equidistant, and just large enough to admit the cord used. By one of these holes it is secured to the elastic. A well padded perineal band is now applied, to the upper part of which a separate piece of cord is fastened. This is brought over the pulley at the upper end of the splint, and then made to perforate successively the two remaining holes in the piece of wood attached to the elastic. It will now be evident that, by slipping this piece of wood up the cord fastened to the perineal band, the elastic will be put on the stretch, and, in this way, extension may be regulated to a nicety.



The limb may now be brought into position, and extension applied as far as is deemed advisable. It does not appear necessary to reduce the fractured limb to its normal length on the immediate application of the splint; for, if this be not directly obtained, the constant extension which is kept up gradually overcomes the contraction of the muscles; and, with very little adjustment, the limb will be found to have gained its proper length in the course of two or three days, the extension being increased or diminished according to the necessities of the case.

There are many advantages besides this. Apart from doing away with the forcible extension necessary to overcome spasmodic contraction of the muscles, which often gives rise to so much pain and subsequent excessive irritability of the parts, there are many reasons for urging the trial of the above-mentioned plan, more especially in fractures of the thigh.

With the exception, perhaps, of an occasional twist once or twice round the knee or ankle to steady the limb, no bandaging is required; or a sand-bag on the inner side of the extremity will generally effect all that is wanted. With the dispensing of bandages, not only is the patient relieved of much pain at the time of putting up the fractured limb, but also of all distress occasioned by their subsequent renewal and adjustment. The surgeon's time is saved, which is no small gain if he has several such cases to attend to daily. The large surface of skin remains exposed. Thus its excreting power is unimpaired; and the irritation which is occasioned by its excretions being pent up, and which at times becomes almost unbearable, is altogether avoided. The limb can be cleansed daily without interfering with the appliances, and every practical surgeon knows the value of this measure. Moreover, with a momentary glance, it can be ascertained whether the position of the limb is good, and any irregularity remedied.

Another very great advantage is, the being able to place the splint and perineal band on the sound side, as has been for some time practised by Mr. De Morgan in cases of hip-disease. This, of course, will be especially useful when there is abrasion or bruising of the injured side, which would be aggravated by anything coming into contact with it. When the splint is used in this way, a sand-bag on each side of the limb will be sufficient to steady and support it. With the old Liston-splint, and its mode of application, requiring, as it at times does, great force in reducing the fracture, and then even failing to obtain proper length, it has frequently occurred to my mind that the separated fragments are often either too far apart, or form with each other a more or less obtuse angle.

The amount of force used, and the mode of fixing above and below, seem almost incapable of bringing the ends of the fractured bone into close and perfect apposition. The present mode of using the elastic is calculated, I think, to ensure this. The extension is equalised; the power above being as great as that below the seat of fracture, over-lapping is more easily corrected, and, as the contraction of the muscles is overcome, the elastic, by its contraction, tightens the cords, so that sufficient extension is always kept up. As the limb lengthens, the elastic of course shortens; and this shortening of the elastic is very essential; for, as the limb attains its normal length the extension ought to be diminished, otherwise the fractured ends might possibly be pulled too far apart. This required diminution of tension is brought about by the lengtnening of the limb. Both objects are effected at the same time, the one by means of the other. When once the separated ends of the bone are placed in good apposition, the length of the limb is easily maintained, slight tension being required to steady them on the one hand, while the contraction of the muscles assists in doing so on the other. The result is close apposition of the parts. This, of course, facilitates and hastens union, and prevents anything like shortening.

Mr. Nunn is of the opinion that, by means of this extension, the normal co-ordinated tension of the muscles is able to keep the bones in proper position.

The appended cases will show with what results this mode of elastic extension has been used. If space allowed, several could be added, besides which we have many more now under similar treatment, all promising like success. This plan could not, of course, be adopted in cases complicated with delirium of any kind; but there appears no reason why it should not be tried in oblique compound fractures, where so much difficulty is experienced in their reduction.

Case 1.—James E., aged 16, was admitted under Mr. Nunn's care on January 11th. Right leg short by an inch and a half; foot everted; oblique fracture of femur at junction of lower with middle third; very considerable effusion and tension around seat of fracture; swelling around, and effusion into knee-joint. Elastic extension on sound side, owing to an ulcer in the right groin; ice to knee; proper length obtained. Feb. 12th. Splint removed; firm union, but slight thickening; length perfect.

Case 2.—Charles H., aged 13, was admitted February 6th, under the care of Mr. Moore. Right leg short by two inches; foot everted: crepitus at junction of middle with upper third of right femur. Elastic extension was applied. Shortening less by one inch. February 7th. Shortening less by one inch and a half. February 9th. Length normal. March 9th. Splint removed; firm union; length normal. Discharged March 10th.

Case 3.—Thomas K., aged 44, was admitted under Mr. Nunn on January 12th. Very oblique fracture of tibia, with fractured fibula; lower fragment of tibia riding, but thinly covered by skin; considerable ecchymosis and swelling. Ordinary back iron-splint was applied; remained on this splint fifteen days; constant adjustment and alteration required to keep fragments in position, but without success. Elastic extension. Fragments reduced to proper position. In three weeks, there was firm union; line and length of tibia quite normal, but slight thickening of tibia to indicate seat of fracture.

Case 4.—John W., aged 43, was admitted under the care of Mr. Nunn on January 10th. Oblique fracture of tibia at middle third, with fractured fibula; lower fragment drawn up above upper one. Back splint and lateral splint were in succession applied, with very unsatisfactory results. February 7th. Elastic extension applied. Fragments easily reduced to their normal position. On the 21st, the splint was removed. Firm union; length normal; line of tibia almost uninterrupted. On the 27th, he was getting about with the help of sticks.

Case 5.—William C., was admitted on February 19th. Transverse fracture of tibia and fibula about four inches above the malleoli; considerable displacement, with shortening and bruising. Elastic extension was applied on March 18th. Firm union; but slight thickening on inner side of tibia to indicate seat of fracture.—*British Medical Journal, July* 20, 1867, *p*. 42.

46.—ON FRACTURES OF THE NECK OF THE THIGH-BONE.

By Samuel Solly, Esq., F.R.S., Senior Surgeon to
St. Thomas's Hospital.

[In Guy's Hospital museum are contained a number of specimens of united fracture of the neck of the thigh-bone. These were presented by Mrs. Hodgson, the widow of Mr. J. R. Hodgson, of Chesham, Bucks. They had been previously described by him in the Guy's Hospital Reports.]

The treatment in these cases was the use of the double inclined plane. The plane was made of pillows sewed together; the foot was fixed to the sacking at the foot of the bed by a handkerchief folded round the ancle and over the instep and tied to the sacking. The thigh and leg were fixed by shawls folded obliquely round the limb above and below the ham, and secured to the under pillow of the inclined plane. This was maintained for fourteen or sixteen weeks.

I attribute Mr. Hodgson's success in these cases to his not disturbing the impacted fragments. His words show this. "I would also remark that, in making out the diagnosis, it is not at all necessary to feel crepitus." The whole paper is worth perusal, and as I was not aware of its publication until I went to the college to examine the specimens of this injury, I feel sure that I am not wasting your time in calling your attention to it now.

I will next give the history of a case which, though not illustrated by a post-mortem examination, is, I consider, equally conclusive as to the possibility of obtaining union in these fractures.

About five years ago I had under my care in private practice a well-marked case of this injury. The subject of the accident was a lady between fifty and sixty, but in some respects old for her years. She slipped on a flagstone in the garden, and fell suddenly on the right hip. She was carried with some difficulty into the house. A general practitioner in the neighbourhood was sent for, and, without making a sufficiently searching examination, perhaps from false delicacy, said that he would go home and get his pulleys. The lady, who was a very intelligent woman, not liking the idea of the process, then suggested that I should be sent for. This was acceded to at once. The general practitioner met me at the station, and told me the injury was a dislocation on to the pubes. Now, although as a rule, fracture of the neck of the thigh-bone is the lesion which we have most reason to expect in old age when the injuring force is applied to the hip, I have seen dislocation on the pubes in a man aged seventy. He was the rector of Seaford, and I was called to the case by my friend Mr. Sanger, of Alfreston. With the help of pulleys and muscular power from the coast-guard station, we managed to return the bone into the socket. Our reverend friend got quite well, and lived four or five years in the perfect enjoyment of a sound limb.

On examination, I found no dislocation, very slight shortening, extreme pain on the slightest attempt to move the thigh-bone at the hip-joint, slight yielding at that spot, but no positive crepitus. I handled it very gently, for being now convinced that there was fracture of the neck of the thigh-bone, I took care not to displace the fragments. I treated her on the plan I have described. I moved her once a week from the couch to have it cleaned and made comfortable, invariably lifting the patient myself; and by keeping one hand over the trochanter I prevented the slightest movement taking place. At the end of eight weeks I allowed her to sit in a chair, and in another week to stand. At first there was very little motion in the joint under the action of her own muscles. On moving it with my own hands it appeared to lock, like a hinge of a door into which a nail or screw has accidentally slipped, stopping the normal circle of motion. This impediment evidently arose from the amount of uniting callus which had been poured out, interfering with the hinge. I employed passive motion gently but freely, and I was rewarded by seeing her at last walk perfectly. There was not more than a quarter of an inch of shortening. Her sister told me that at the end of nine months she astonished and frightened her by saying, "See, Mary, what I can do," and then running down a flight of stairs like a girl of eighteen.

Thirty years before this accident had occurred the then proprietor of the house, which was a fine old family mansion in the country, was the subject of the same injury. He was a patient of Sir Astley Cooper, and lived several years after the injury, but never recovered the use of his leg. His injury most probably differed from the case just related in being a fracture within the capsule, and would therefore be more difficult to cure. No surgeon, however skilfully he may treat these cases, can *secure* union; but that is no reason for not doing our best to effect it.

I was young in the profession when I learned, from personal experience, that you may examine a case of this kind of fracture and not discover the nature of the injury. My patient was an oldish lady—by oldish I mean about sixty—who actually walked into my consulting room. There were none of the usual symptoms of fracture—I suppose from its complete impaction—and I did not treat it as such. She was very anxious to go down to Edinburgh, to which I gave my consent. When there she was worse, and consulted Mr. Lizars, who detected fracture; and when he heard that I had pronounced a different opinion, he said, "Yes, it is very easy for me now to detect this injury, for the bones are in a very different condition to what they were when Mr. Solly saw you." The journey to Edinburgh had jolted them asunder. I do not think I have made the same mistake since, and my principal guide has been the severity of the pain and its persistence.

In some of these cases of fracture there is considerable shortening, and it has been recommended that the limb should be drawn down by means of a weight hung over the end of the couch. This manœuvre must be adopted with great care, lest you detach the fractured portions; and you must remember that a shortened but firm limb is better than an ununited fracture and a feeble, almost useless limb.

This beautiful specimen of united fracture of the neck of the thigh-bone was given me by a surgeon, who witnessed the accident, traced it at the time, and examined the joint after death, which occurred about ten years subsequently. The patient wore an extra sole 1¾ in. thick to his shoe on this side. This drawing I made from the specimen, and have since deposited one half in our museum, and the other half in that of the College of Surgeons. I give the history, a little curtailed, which I have received.

S. N., aged 44, bricklayer, fell into a cesspool, from the giving way of a slate which he had placed across the top. The left leg went down first, and the right leg was doubled up under him. Surgical attendance was immediate. The appearances presented were those of dislocation on to the hip. This was reduced by the aid of pulleys, and the patient placed in bed. About an hour afterwards he complained that, though he had kept perfectly quiet, the bone was out again. On this occasion it was clear that the injury was a fracture of the head of the thigh-bone extending into the trochanter major. Now, my friend who has given me this account, a very good surgeon, firmly believes in this order of events. I doubt it, and believe that it must have been a fracture from the first, the projection of the displaced trochanter giving the appearance of dislocation. I have seen the neck of the thigh-bone broken by the surgeon during a futile attempt to reduce a dislocation; but when that occurred the dislocation was not reduced, and the fracture was transverse, not impacted. The accident was recognised immediately. I feel certain that it would not have occurred in the case without my friend immediately discovering the mischief he had done.

R. W. Smith, speaking of these cases and the difficulty of diagnosis, says:—"I have seen seven examples in which the foot was turned inward, in five of which the fracture was external to the capsular ligament. Such cases merit the closest attention, for they are especially liable to be confounded with luxations. I have seen this mistake committed more than once. . . . Mr. Stanley has tersely observed that among the more complicated injuries to which the hip-joint is liable, that of fracture of the trochanter major, combined with fracture of the neck of the femur, has, under certain circumstances, a strong resemblance to dislocation of the head of the bone. . . . When, from the direction of the fracture, one portion of the trochanter has been drawn by the action of the muscles towards the great ischiatic notch, no crepitus will then be discoverable. A direct source of mistake will then arise from the positive resemblance of the fractured portion of the trochanter to the head of the femur; the former occupying the same place which the latter would do in dislocation; and if with these circumstances there should happen to be inversion of the injured limb, the difficulty of the diagnosis must be considerably increased."

This same author narrates a case of a middle-aged man falling in the street, his hip striking the kerb-stone. The immediate consequences were that the limb was inverted and shortened to the extent of an inch, and no crepitus could be discovered. It was presumed that a dislocation had occurred, and accordingly an extension of the limb was made. The constitutional irritation occasioned by repeated trials to reduce the supposed dislocation was such that the man died five months from the time of the occurrence of the accident. In the dissection of the hip a fracture was found, extending obliquely through the middle of the neck of the femur, but entirely within the capsule. A portion of the fibrous and synovial membrane on the anterior side of the neck of the bone had escaped laceration.

We must, after this digression, continue our narrative of the patient. He was placed on an Amesbury, or the double inclined bed similar to that which I now call Alderman's. Complete union took place, with about two inches of shortening, which was to some extent remedied by a thick sole to his shoe. He died ten years afterwards of phthisis. At the post-mortem examination the head of the thigh-bone was found in the acetabulum.

The shortening of the neck, the depression of the head, the immense amount of callus thrown out, some of it extending like a blunt spine in the course of the capsular ligament, accounts for the shortening of the limb, and the impeded motion in the joint.

We will next sum up the signs or symptoms by which you can distinguish intra-capsular from extra-capsular fractures. In giving you these land-marks, do not mistake me, and use them as our forefathers did, to indicate a different line of treatment; or rather I should say a line of treatment in the one case and a line of no treatment, or the *do-nothing* system, in the other.

I have shown, by reference to the preparations in the College of Surgeons' museum, and also in our own, that fractures of the cervix within the capsule will unite, though not so frequently as those without, or partly within and partly without. The treatment must be the same in both. You might, perhaps, as idle students, say, "Why should I bother myself with the distinctions!" For this reason, that in all surgical practice, though, perhaps, it is more important in private than in hospital practice, as your reputation may be more affected in the one than in the other, it is a matter of the greatest importance for any patient, whether a lady of high degree or a working peasant, to know after the receipt of any accident what are the prospects of recovery; whether it is likely they will be able to walk or work again, or whether they will be cripples for life. If you can diagnose that the fracture is an impacted fracture of the cervix, then you may with tolerable confidence predict complete union and a sound limb; but if you have reason to believe that it is a fracture within the capsule, you must give a very guarded opinion, promising to do your best, but that you cannot honestly say that your endeavours will be crowned with success. You must acknowledge the imperfection of your art and the uncertainty of the result. You may also add that such fractures have united, when properly treated, as you intend to treat the case before you. Of course the age and constitutional power of the patient will be an essential element in favour or against the probability of union. The two extremes may be thus put : Fracture within the capsule in a very old and feeble person scarcely ever unites ; fracture without the capsule in a strong and healthy person must be very badly treated if it does not unite.

Fractures within the capsule may generally be distinguished by the fact of a fracture existing being almost unequivocal ; by the signs of shortening ; easy production of crepitus after reducing the limb by traction to its normal length ; by the eversion of the foot ; by the age of the patient being greater usually than in the instance of the impacted form. It is true that inversion of the foot does occur in the impacted, as in the case just related, but still it is not so frequent.

In impacted and extra-capsular fracture the symptoms are often obscure, as I have already shown, so much so as even to deceive well-educated surgeons into the belief that there is dislocation and no fracture. The foot, instead of being everted, is not unfrequently inverted, and all attempts to turn it inwards give rise to excruciating pain. The shortening is scarcely perceptible. Locomotion is not impossible. Patients can often stand and walk, and raise the limb in bed as they lie upon the back.

In the complete fracture within the capsule the limb looks like a paralysed limb, flabby, powerless, and immoveable, with its whole expression altered.—*Lancet, Aug.* 17, 1867, *p.* 187.

47.—REPORT ON THE TREATMENT OF UNUNITED FRACTURES.

[Dr. Bigelow, Professor of Surgery at Harvard University has been extremely successful in the treatment of ununited fractures. The following is a description of his usual mode of operating.]

The extremities of a false joint should be attacked where they approach nearest to the surface, unless vascular or nervous trunks are in the way ; for example, in the arm, upon the outside ; a free incision being contrived in each case with especial reference to the ready exit of pus. In the arm, the musculo-spiral nerve, which is often displaced and tied by the lymph, is to be carefully looked for and avoided, and were it not for the care here requisite the bone might be exposed by a single incision. The principal bony extremities being found, the interval, which is sometimes quite irregular and interlocked, is gradually divided, and the ends turned out, the dissection being materially aided by an assistant powerfully flexing the false joint. Care must be taken to prevent the muscles being stripped from the periosteum. When one extremity is fairly exposed, a crucial or other regular incision is made in the ragged callus over-lying the periosteum at its tip, which should be then seized by strong-toothed forceps, and efforts made to tear it out of the rugous inequalities of the formerly inflamed bone. As the flaps begin to yield, the terminal adhesions are detached, and the sound bony shaft is reached, where the periostum is easily stripped from the bone, requiring great care lest the shaft should be denuded higher than the intended section.

The soft tissues being now protected by spatulæ, or flexible strips of copper, the end of the shafts is removed by a common saw, the length of this fragment being determined by the amount of periosteum it has been necessary to detach. Half an inch of good cylindrical periosteum, with half an inch more of ragged tissue hanging at its extremity, has usually covered from three quarters to an inch and a half of bone. Perhaps half an inch of sound shaft, with an irregular or conical extremity varying in extent, is a good rule for the excised piece in most cases. The other extremity is now to be turned out and treated in the same way, and this terminates the dissection.

There now only remains the wire to be inserted. For this purpose holes are bored in each extremity with a good bone drill, larger than the wire, at a little more than half an inch from the end, and through one wall only. A pure silver or plated copper wire is inserted from without inward in one end, and inversely entered in the medulla from within outward in the other. The ends are brought together accurately, and the wire twisted long enough to protrude at the external wound. It is now only necessary to bring the edges of the wound together, leaving an aperture large enough to allow of the free escape of pus, and then to apply a suitable apparatus for retaining the proper position of the parts.

. The apparatus for the humerus used by Professor Bigelow, consists of a firm concave splint of iron and leather, made to fit the top and outside of the shoulder as low as the axilla, and thence horizontally to the neck. This is secured by a strap around the opposite axilla. A similar gutter receives the elbow and fore-arm flexed at a right angle; the two are united by a narrow iron strap on the back, and another on the front of the humerus, adjustable as to its length. The splint can be thus shortened when in place, so as to keep the extremities of the bone in contact, and nearly immovable, in spite of the great leverage of the arm upon the wire, while the dressing can be readily applied in the open interval, without disturbing the apparatus.

For the thigh, a pasteboard splint is moulded to the anterior aspect of the thigh and leg, and then stiffened with dextrine, an interval for the wound being left. The whole limb is then secured to this by bandage; and surmounting the whole, a

Smith's anterior splint is applied, by which the leg is suspended from a railway on a framework over the bed.

Professor Bigelow employs water-dressings at first, and poultices or oakum to absorb the discharge afterwards. He keeps the patient in bed for several weeks, in fact till some stiffening has taken place, after which he prescribes fresh air as an invigorating and osteoplastic agent, together with a diet as generous as the appetite will bear, and not unfrequently the use of phosphates.

The wire is allowed to remain in place until the bone is firmly united, generally during several months; in no case has evidence of any ill effects from its presence been observed either in producing necrosis or undue inflammation. In some cases it has remained quietly in its place after the arm was in use. In one instance it remained for two years.

To remove the wire, the loop was divided with cutting pliers, and forcibly drawn out; hence an advantage in flexible wire. This loop is sometimes quite superficial, but in other cases is so deep as to require an incision to reach it.

It has been observed that a partial stiffening, dependent on the inflammation of the soft parts, sometimes takes place in a few weeks, but the bone afterwards becomes gradually loose if the periosteum fails to do its duty.

The one great point in treatment, on which Professor Bigelow has uniformly insisted in his lectures, is the prevention of abscess, or, in other words, the early and free evacuation of imprisoned pus, by large and dependent incisions, which here, as elsewhere, are less injurious to the tissues than the burrowing of pus. Again, the formation of an abscess is always attended with a certain amount of fever, and the consequent loss of appetite and debility, may materially interfere with the reparative process.

From this circumstance the first signs of inflammation should be vigilantly watched for, and an opening should in all cases be made as soon as any evident pointing occurs.

The most favourable cases for the operation are those in which the patient is in vigorous health, and the bony extremities are of the natural size. In chronic cases the ends of the bone are often tapered from a sort of local atrophy. On this account the incisions may have to be longer, and the whole operation more formidable.

Softening of the bone from interstitial absorption may be looked upon as contraindicating the operation, since we find that the only case in which Prof. Bigelow has completely failed, is one in which that unfavourable condition existed, and could not be removed by any treatment, including of course, the free use of phosphates. There is one accident incidental to some of these operations which occasionally supervenes and demands a moment's consideration. We allude to the supervention of paralysis from division of, or injury to, the musculo-spiral nerve. The paralysis will, of course, be more or less complete according to the extent to which the fibres of the nerve have been severed. As the nerve winds around the outer side of the arm it is difficult always to avoid it, and this difficulty is increased in those cases in which deformity has to some extent displaced it. Masses of indurated lymph may also enclose the nerve. It may also be inadvertently injured where so much congestion exists that the wound cannot be kept dry during the dissection. It has further been found at times concealed in a groove of new bone, in separating which it must be divided. This accident has occurred to Professor Bigelow twice. In both cases the power of the paralysed muscles was ultimately restored. It is but fair to remark that the same accident may equally occur in the course of the other operations that have been recommended for the cure of this lesion. In cases of gunshot wound so much damage is often done to the bone that necrosis is almost inevitable. In reference to this we may remark that Professor Bigelow seems to consider his operation as almost contraindicted where there is complete necrosis, and most surgeons will probably consider other methods of treatment more appropriate to such cases.—*Medical Press and Circular, Aug.* 21, 1867, *p.* 175.

48.—ON EXCISION OF THE ANKLE JOINT.

By Henry Hancock, Esq., Surgeon to Charing Cross Hospital.

[Resection of almost every joint except the ankle has been carried into practice, yet there is no reason why so valuable a member as the foot should be sacrificed, as it is when Syme's or Pirogoff's operations are performed, when simple excision would suffice.]

I have now performed this operation five times—four times successfully, once unsuccessfully, the patient dying some six months after the operation from lung disease, the result of a life of dissipation. In no instance has there been sloughing. There need not be a single tendon or artery divided. There is afterwards very little deformity—comparatively little shortening; the foot is preserved, and, as we shall see, the patients are able to walk and run with perfect ease. Surely an operation presenting so many advantages should not lightly be cast aside nor condemned by theoretical objections without a trial. But so little consideration has there been given to the matter, that in England, even in these days of conservative or pathological surgery, as many as thirty-four legs were amputated in one year for disease reported to have been restricted to the ankle-joint, without even the improvements afforded by Syme's or Pirogoff's methods having ever been attempted. Very exaggerated notions appear to prevail with reference to the difficulties attending the performance of this operation.

Mr. Furneaux Jordan, of Birmingham, whose opinions merit our greatest respect and attention, has kindly favoured me with the following observations :—"A very strong objection to excision of the ankle is this, that the astragalus is not a long bone with an epiphysis in which the chief disease may lie, but a short bone consisting of a mass of cancellous tissue, throughout which the disease is more or less diffused. And as in the ankle disease only rarely extends from the synovial membrane to the bone, the disease here, as in all allied cases (the extremities of long bones for instance), begins in the centre of cancellous tissue, and consequently can only be removed with the whole bone. I have seen two cases only ; both confirmed the conclusions here arrived at. The pathological inferences are the result of a great number of examinations of cases in the ankle and foot. Of course, I infer that when disease begins in the extremities of long bones excision is inadmissible.

The histories given by those labouring under disease of the ankle-joint would lead us to infer that disease commences much more frequently in the synovial membrane than is here allowed by Mr. Jordan ; and if we turn to the writings of Mr. Syme, we find that eminent surgeon dwelling upon the frequency of the disease originating in the joint between the calcis and astragalus as an argument in favour of his operation. We do not for one moment deny that in patients of strumous habits the disease, for the most part, begins in the cancellated tissue either of the astragalus or of the ends of the long bones, or of both. What I would urge is, that it is not by any means an invariable rule that persons suffering from ankle-joint disease are scrofulous, and that when this condition does obtain it is no argument against the operation of "excision of the ankle-joint."

Granted that disease commencing in the centre of the cancellous tissue of the astragalus necessitates the removal of the entire bone, is this a sufficient reason for sacrificing the whole foot ? I have, as you will find in one of my cases, removed not only the entire astragalus, but a considerable portion of the os calcis also, and still the patient recovered completely. Nor am I disposed to admit the inference that when disease begins in the extremities of the long bones excision is inadmissible. It is not so admitted in excisions of the knee-joint ; why should it be so fatal an objection to excision of the ankle ? We have here the epiphysis of the tibia, which frequently sets limits to the disease; and the increase in the amount of bone to be removed is a matter of small importance indeed, when compared with the benefit conferred upon the patient by the preservation of the foot.

With regard to the mode of performing excision of the ankle-joint I would offer a few suggestions which appear to me of practical importance.

Some surgeons make their incisions through the skin according to the plan proposed by the elder Moreau—namely, a longitudinal incision behind each malleolus, with a short horizontal one connected with it at right angles below the process. The late Dr. Buchanan, again, limited his incisions to one extending from the peroneus tertius to the tendo Achillis, whilst he attacked the joint entirely from its outer side. I have never seen the operation performed in this way, but I have seen it done according to Moreau's plan, and I infinitely prefer the one semilunar incision extending across the front of the joint to behind the malleoli on either side, penetrating only to the fascia below, as in my own proceeding. When the flap thus made is reflected we have plenty of room wherein to manipulate; we can see the condition of parts and what we are doing; we can detach the tendons sufficiently from their grooves behind the malleoli to do away with the necessity of cutting them across; we greatly facilitate the eversion of the foot; and, as we can see what we are about, we are able to avoid wounding the posterior tibial artery—a point of vital importance to the success of the proceeding, since it mainly depends on the anterior and posterior tibial arteries being preserved in their integrity. The advantage of this method, moreover, is greatly enhanced when, from long-continued disease, the soft parts around the joint are rendered thick and unyielding.

Again, if we meddle with an ankle-joint at all, no matter how limited the disease may seem to be, it is better to excise the entire joint than to leave any portion behind. It is true that I shall have presently to relate a most successful case of Mr. Wood's, which seems to controvert this doctrine; but this case is so exceptional in its results that it proves the general rule and the soundness of the proposition. It is not always easy to judge from the appearance of the surface to what extent the mischief in the cancellated structure has proceeded; and however carefully and completely we may endeavour to gouge the disease out, we rarely succeed so entirely that some portion is not left behind. In partial excision we expose our patients to the dangers attending wounds of joints wherein so much mischief occurs from the impediments to the free exit of matter; in complete excision, on the contrary, we free the entire joint, remove these impediments, and get rid of the joint structure—bone is brought in direct contact with bone, and the process of cure is rendered more certain and more simple. These observations apply with equal force to those cases in which one or both of the malleoli have been allowed to remain.

Mr. Statham excised the astragalus. A year afterwards he removed the two malleoli. The parts never healed, and the man suffered so much that at his own request Mr. Bowman amputated the leg three years after the first operation.

Mr. Price gouged away the astragalus and horizontal surface of the tibia, and left the malleoli. Eight months after he was obliged to operate a second time.

In another case Mr. Price gouged away the astragalus and articular end of the tibia, the posterior end of the cuboid, and a portion of the fibula. Three months afterwards he was obliged to amputate the leg in consequence of abscess.

In June, 1852, Mr. Solly gouged away the external malleolus, a portion of the astragalus, and the horizontal articulating surface of the tibia of the left foot. On July the 2nd he performed a similar operation on the right foot of the same patient. On October 2nd he again operated on the left foot, and in April, 1853, he again operated on the right. The patient, however, ultimately recovered. On June 18th, 1852, Mr. Solly gouged away carious bone from the end of the tibia and from the astragalus. On July 10th he was obliged to amputate the leg.

In Dr. Buchanan's case the external malleolus and astragalus were removed, but the articular surface of the tibia was "scooped out with a gouge, and the internal malleolus left." The patient died of phthisis.

The younger Moreau in the year 1796 gouged away from a youth, aged 17, the astragalus, the internal malleolus, and about an inch and a half of the lower end of the tibia. "Above this," he says, "the solid part of the bone was sound, but not so the cancelli, which were so much diseased that I was obliged to introduce the gouge, and take away about two inches of them. The result was not so favourable as was expected. The wounds were a long time in healing; the fibula, having no assistance from the tibia in sustaining the weight which it was obliged to support, sank down upon the external side of the foot, which was thrown inwards, so that the lad rests now on the outside of his foot; that, however, does not prevent him from walking. I allowed the inferior extremity of the fibula to remain, thinking that Nature, by reproducing that portion of the tibia which I took away, would finally give by both bones a solid support to the patient; but the event was not what I expected. In a similar case now I would cut off as much from the fibula as from the tibia."

Mr. Hussey also, as we have seen, sawed through the fibula and removed the external malleolus, but he gouged away the horizontal portion of the tibia and upper surface of the astragalus, leaving the internal malleolus. Six months afterwards he was obliged to amputate the leg.

The cases here quoted appear to me very conclusively to prove my proposition, that if we meddle with the ankle-joint at all we should remove it entire, and not a portion only of the joint; as, considering the comparatively few times that these operations have been performed, the results have been most grievous when compared with those wherein the opposite plan was pursued. And I would add that, where practicable, the bones should always be removed by the saw, and the employment of the gouge avoided as much as possible. With the saw we are enabled to cut through the bone with very little violence to the cut surface, which is smooth, and for the most part retains its vitality. But with the gouge these advantages are lost; we never can tell with any degree of certainty whether some amount of disease may not be left behind; we inflict an amount of violence, bruising, and crushing which the parts, already weakened by the pre-existing mischief, cannot repair: and thus the operation fails. The disease either proceeds unchecked or with increased rapidity, or, if arrested, the parts are too much damaged to take on the process of reparation. When this occurs it is commonly ascribed to extension of the mischief, to want of power in the patient, to the existence of scrofula or some other taint in his system or constitution; but that these supposed causes are not always the true ones is strongly exemplified in the case of the foot amputated by Mr. Hussey. Here the end of the fibula, separated by a clean cut of the saw, was surrounded by a large mass of bone structure, thrown out, there can be but little doubt, from the periosteum and cut end of that bone itself; but the gouged opposing surfaces of the tibia and astragalus, we are told, were rough, without any deposit of new bone and without any appearance of commencing anchylosis. We must therefore admit, in this case at all events, that either the diseased portions of the tibia and astragalus were not removed at the time of the first operation, or that the injury done to the parts was so excessive as to be beyond their power of reparation. The result cannot be charged against the weakness of the patient, or the presence of scrofula or some other taint in his constitution. Had either of these latter reasons obtained, it would have affected the fibula equally with the tibia and astragalus; and we should scarcely have had an entire absence of all effort at reparation in the latter, whilst in the former the process not only of recovery but of reproduction progressed so vigorously and abundantly.

I have very little doubt that when this operation is better understood, and the cases to which it is applied are more carefully selected—that when the vast benefits it is capable of affording to the patient are more extensively known and appreciated, the prejudice with which it has hitherto been regarded will fade away, and that it will be more generally adopted, and take its proper position in the rank of the standard operations in surgery.

We have seen that of the seven cases which terminated fatally, four died of consumption, and one of syphilis. Such cases can scarcely be adduced as evidence against this proceeding, as death would have equally followed any other operation to which they had been submitted. Even after the most patient and careful inquiry we cannot always obtain a satisfactory previous history, or one on which reliance can be placed. Patients too frequently deceive the surgeon, and deny or conceal facts of great importance in relation to their ultimate recovery, and it is only when too late to repair or to obviate the mischief that the truth is revealed. This was the case with the patient on whom my friend and colleague, Mr Barwell, operated for excision of the ankle-joint. All who know Mr. Barwell are aware of the great care and talent which he brings to bear upon the practice of his profession; still, in spite of his inquiries and caution, it was some days after the operation had been performed, and when too late to remedy the mischief, that the patient admitted that which too surely proved her destruction. If therefore there be any trace of syphilis neither this nor any other operation should be performed. In Mr. Barwell's case the parts bled very freely when first incised, nor did the pressure made by Mr. Hird on the femoral artery produce any appreciable diminution of the hemorrhage; but all the surfaces ceased bleeding after they had been exposed a few minutes to the air. Successive hemorrhages took place from day to day till she died, ten days after the operation, and it was remarked that the blood discharged seemed to be putrefied, and had a most offensive odour. It was not till after her death that the fact of her having suffered from syphilis transpired, as there were no general symptoms of the disease. As bearing on this subject, I would allude to a valuable and interesting case related by Mr. Cæsar Hawkins before the Pathological Society on Oct. 15th, 1850:—

"The joint between the os calcis and astragalus was partially anchylosed, and *although the astragalus itself was quite healthy* the joint between it and the tibia showed the commencement of ulceration of the cartilage, demonstrating the manner in which the inflammation of the soft parts sometimes conveyed disease from one tarsal or carpal joint to another, and causing the removal of pieces of necrosed bone. or the excision of even an entire bone, to be not always successful." The case was remarkable however, more from its history, as illustrative of the difficulty occasionally observed in distinguishing between simple scrofulous and malignant affections. The patient, a young woman, was admitted into St. George's Hospital with a large phagedenic ulcer of the inside of the ankle, excessively painful, and requiring large doses of opium for its relief, with much thickening of the soft parts; looking, in the sections, not unlike scirrhus, and having in the centre some warty granulations, resembling the cancer of cicatrices (of which Mr. Hawkins has written a description), in the centre of which the probe passed down into the joint, between the astragalus and os calcis, without touching dead bone. After some attempts to heal these parts, amputation was proposed, but declined by the patient. After a few months a mass of glands appeared low down in the groin, as large as an orange, as if enlarged by the contamination from the apparent cancer below, which did not ulcerate like strumous glands, but sloughed gradually away after previous ulceration of the skin; and, as some were destroyed, fresh masses formed by their side, which went through the same process of sloughing during two or three months. About the same time there formed at the lower part of the femur, on its inside, a firm elastic tumour, obviously connected with the periosteum, but without bony deposit, about four inches in length and perhaps two inches in height. Soon after this a growth took place on the left side of the frontal bone, soft in the centre, and consisting apparently of a morbid growth in the diploe, making its way by pushing out the outer table, with a little absorption of the bone in the centre. Thus was formed a continuation of appearances seeming to confirm the supposition of the malignant nature of the original disease, and quite sufficient to deter Mr. Hawkins from operating, for which the woman was now desirous. In two or three months, however, and, as it seemed, chiefly from the use of iodide of potassium and sarsaparilla, all these latter growths disappeared; the tumour of the thigh gradually subsided, the glandular masses all sloughed out, and the part healed; the growth of the diploe disappeared, the bone recovered its former level, and the skin lost its former sallow hue. Amputation was performed, and the patient was quite well a year and a half afterwards.

Although the above case is described as one of strumous disease of the os calcis, femur, and cranium resembling malignant disease, it seems to me to have been neither the one nor the other, but a very severe case of syphilitic mischief presenting its local manifestations at the various parts indicated; and I am led to this conclusion by a somewhat similar case about which I was consulted some six years ago.

I was requested to see a gentleman residing in the neighbourhood of Blackheath, said to be suffering from malignant disease of the ankle-joint, especially with reference to performing amputation of the limb. He had been suffering for four months, was extremely irritable, anxious, and emaciated; and described his sufferings as almost unbearable, entirely depriving him of rest, so that, as he said, he was almost worn out. He had no appetite; his countenance was leaden and sallow; and his pulse was extremely rapid, and at the same time feeble. The ankle was much swollen, as was also the foot. Over the inner ankle was a large uneven ulcer, presenting all the appearance of a malignant sore, the surrounding skin being of a bluish-brown red, and the discharge thin and very offensive. He described the pain as a dull, burning, gnawing pain, implicating the bone. But I was much struck with the manner in which he dwelt upon its increasing intensity towards night, adding that it always subsided towards the morning. He denied having ever suffered from syphilis, although he admitted having had a slight excoriation about twelve months before, of which he had not taken any notice beyond touching it with caustic. Under these circumstances I suggested the probability that the disease, after all, was syphilitic rather than malignant; and that it was advisable to submit him to a regular course of treatment for the former disease before having recourse to operation. I accordingly ordered the iodide of potassium with sarsaparilla, and five grains of blue pill every night; and the black wash to be applied to the wound. This gradually healed under the treatment, and the patient recovered completely. He is now walking about as well as ever he did.

Lastly, as regards excision of the ankle-joint, I would lay great stress upon the necessity of ensuring a free exit for the matter; and this is an additional reason for sawing away the whole joint rather than gouging away a portion only. By the former method we ensure a clean, smooth plane across the joint, clear of those inequalities which can scarcely be avoided when the gouge is employed, but which serve as so many barriers behind which the discharge is prone to collect, causing irritation, increasing the discharge, and delaying the cure. It is here that one of the benefits of making the flap, as practised in my operations, is exemplified. By its looseness and freedom we are enabled to ensure the wound behind the ankles being kept open, which we cannot do when the right-angled incisions of Moreau are only employed. These, when the bones are removed, do not correspond to the resulting cavity in the ankle, whilst the flap thus made, free only in a portion of its extent, unable consequently to yield to the swelling and tension of the secondary inflammation, forms a valve, as it were, closing the external outlet, and deprives the patient of one of the principal sources of cure. Whilst, therefore, the wound in front of the joint may be closed and secured by wire sutures, the portion of the wound over the sides of the foot should be left entirely free.

The following case, for which I am indebted to Mr. Tudor, of Dorchester, is a good illustration of the value of preserving these free openings.

H. K., aged 23, a Hanoverian and a sailor, was admitted on board the *Dreadnought* for the second time on the 13th of April, 1859, with disease of the ankle of two and a half years' standing. The previous history, in March, 1858, when first admitted, was an injury to the foot, which was jammed between two ships. He continued to work upon it for about a year, when he first felt painful pricking sensations in front of the joint, accompanied with swelling. He was treated for eight months in the British hospital at Buenos Ayres without benefit; was sent home to England, and admitted on board the *Dreadnought*. The ankle was then very much swollen and painful, but there was no wound nor sinus. He remained in the hospital four months, and was discharged much better, but still unable to use the limb. He was re-admitted on board the *Dreadnought* on the 13th of April, 1859, when his ankle was found swollen and puffy, with fluctuation, and a distinct grating could be felt between the astragalus and tibia. There were now two sinuses discharging pus, one situated in front of the joint, a little external to the mesial line, through which a small patch of denuded bone could be felt on the upper surface of the astragalus, whilst the second, situated midway between the tendo Achillis and external malleolus, impinged upon the same point from behind.

June 9th. Under chloroform an incision was made in front of the joint from the external border of the extensor communis in a curved line, about an inch behind the external malleolus. About an inch of this bone was sawn off, and the ligaments, as well as the tendons of the peronei muscles, were divided. The external and anterior surfaces of the astragalus were found denuded of cartilage, and the bone diseased. About three-fifths of the bone were removed by saw and gouge, and the parts quietly brought together with sutures.

11th. The wound looking red and congested, with a tendency to slough, the sutures were removed, and free openings made for the discharge.

Healthy inflammation soon set up, and the man's health quickly improved, and he subsequently left the hospital with every promise of a useful joint.

Although as a general rule I strongly advocate the removal of the entire joint in preference to a portion thereof, I am free to admit that exceptional cases have occurred where "partial excision of the ankle-joint" has been attended with success. The case—the most worthy of record with which I am acquainted —is the following, operated upon by Mr. Wood, at King's College Hospital:—

A single woman, aged 25, was admitted June 15th, 1858. Ten years previously she slipped and sprained her right ankle very severely, for which she was an inmate of Guy's Hospital for three months. The joint became stiff, and she could not walk upon it for twelve months. She was then for three years under the care of the late Mr. Lonsdale at the Orthopœdic Hospital, during which time an abscess opened in front of the internal malleolus. This never healed. Some time after this she received a kick on the heel of the affected foot, which caused great pain and swelling.

When admitted there was considerable swelling of right ankle; the foot was slightly movable, but the attempt to move it gave her extreme pain. Three sinuses, left by abscesses, communicated with diseased bone, and passed upwards to the tibial portion of the joint, the largest being in front of the inner malleolus, a second, placed posteriorly, passed between the tendo Achillis and posterior tibial vessels and nerve, whilst the third, placed behind the outer malleolus, was directed inwards and forwards towards the tibia. An oily fluid appeared occasionally with the matter upon the dressings.

On July 29th the patient was placed under chloroform. Mr. Wood made an incision two inches long over the inner malleolus, and by gouge and forceps removed the whole of the diseased inner malleolus, exposing a piece of necrosed bone of the size of a chesnut, which included the greater portion of the tibial articular surface, completely denuded of cartilage, which latter lay detached in the cavity of the joint. This was removed, with a portion of gelatinous synovial membrane. The rough articular surface of the astragalus being removed with bone forceps, the bone was found to be quite healthy.

The patient rapidly recovered. The foot had a great tendency to turn inwards, from the lack of support caused by the removal of the inner malleolus, whilst the fibula remained in its place.

On Nov. 23rd, 1859, Mr. Wood again saw this patient. She had become fat, and said she had never felt better in her life.

All the sinuses had quite healed. The leg is about three-quarters of an inch shorter than the others. She walks well and firmly, and with the slightest possible limp. The ankle is quite firm and stiff. A deep hollow marks where the malleolus formerly existed, and the inner arch of the foot is somewhat elevated and shortened. She says she can walk six miles without feeling tired, and that she never has pain in the ankle-joint.

Mr. Wood may well be congratulated on the result of this case, which is almost unique, and unusually successful. Nor must I omit to mention a somewhat similar case operated upon by Mr. Moore, at the Middlesex Hospital, on the 2nd of March, 1855.

A pale thin clerk, aged 21, had disease of the ankle early in life, but the foot, though stiff, had been useful and free from active mischief for the last eight years. Eight weeks before an abscess formed, and he was admitted into the Middlesex Hospital, under the care of Mr. Moore, with three sinuses inside the ankle. There was much swelling over the lower part of the tibia and inner side of the joint, a little in front and behind, and none outside. The foot was extended and fixed. After the failure of treatment, Mr. Moore made an angular incision down the back of the tibia, along the lower edge of the inner malleolus, and across the front of the joint, through the tendons and anterior tibial artery. The joint was thus exposed from the tendon of the tibialis posticus to that of the extensor longus digitorum, which were not divided. On removing the internal malleolus, the diseased surfaces of the tibia and astragalus were exposed, and they were each sliced off quite smoothly, in a single cut, with sharp and large cutting pliers. The fibular part of the articulation was not interfered with, as it was quite fixed, and the whole of the diseased parts appeared to be removed. The result was that a gap was left between the end of the tibia and the top of the astragalus. There was little constitutional or local irritation, and the cut surfaces of bone granulated healthily. A fortnight after the operation there was some redness and tenderness over the external malleolus, with pain, which kept him awake one night. This all subsided. In another fortnight a bare piece of bone was felt on probing to the depth of the inner surface of the fibula. A month afterwards it appeared to have separated and been discharged, for the patient spoke of a pricking sensation which crossed the joint upwards, and on further probing no bare bone could be felt. By the end of May there was scarcely any discharge, and he soon after went into the country, his ankle feeling strong, though the sinuses were not quite closed, and his health being good. *Lancet, June* 15 *and July* 27, 1867, *pp.* 729, 91.

49.—ON CICATRICES ADHERENT TO BONE AFTER AMPUTATIONS.

By H. HANCOCK, Esq., Surgeon to Charing Cross Hospital.

[The two following cases show how important it is to examine whether a cicatrix adheres to the bone underneath, so as to cause the excessive pain which we find in some stumps.]

The first instance was that of a lady betwen 50 and 60 years of age, to whom I was called in the year 1857, by my friend, Mr. Chapman, of Hounslow.

Mrs. B., when 30 years of age, suffered from suppression of the catamenia, for which she was on several occasions bled in the leg. After the last bleeding pain occurred in the spot. The pain continued with increasing severity for three years, during which period she was seen by most of the first surgeons of the day. She next consulted the late Mr. Liston, who at once excised the painful spot. The wound healed, and she remained free from pain for several years, when it returned precisely in the same spot, and continued for several weeks very severe, and not relieved by treatment, although there was neither redness nor swelling. Another surgeon of great eminence was then consulted; he proposed to remove the cicatrix, and this was done with relief, which lasted sixteen months, when the pain returned. The cicatrix was again removed, but at the end of six months she was as bad as ever. Her sufferings at this time were so great that she was willing to undergo any operation which held out the probability of effecting a cure. She appeared quite worn out with pain and want of rest. Upon hearing the history of her case I was struck with the fact, that after the last two excisions she remained free from pain so long only as the resulting wounds were open and unhealed, but that directly the cicatrix was completed the pain returned; and when, upon examination of the part, I found that the cicatrix was adherent to the periosteum, and perfectly immovable, I concluded that her sufferings were due to this cause, and that they would not be alleviated so long as the parts remained in their then condition. I therefore proposed that the skin should be detached from the subjacent periosteum by subcutaneous division; that reunion should then be prevented by moving the skin backwards and forwards from day to day, so that the new skin of the cicatrix should be placed in the same position as that of the surrounding integument. This having been agreed to, on the 15th Sept., 1857, I detached the cicatrix from the bone, the part cut through being cartilaginous, and so hard as to require a sawing motion of the knife. Mr. Chapman, who attended the case subsequently, and carefully prevented readhesion, informed me, in 1859, that the patient was then in good health, and had remained perfectly free from pain during the two years since the operation.

The next case was that of a Mrs. H., aged 30, admitted into Charing-cross Hospital on Nov. 30th, 1858. She had disease of the left knee-joint at ten years of age. At fourteen, the knee being much swollen and very painful, was punctured, and a considerable quantity of blood escaped, but no matter. At sixteen the catamenia first appeared; they left her for two years, and then returned with irregularity. At seventeen years of age she fell, and so injured her knee that she went into the Royal Free Hospital, where the leg was amputated. The stump healed rapidly, but accidentally falling on the floor, she hurt the stump so much that it reopened, and the bone protruded through the wound, preventing its healing. The pain then became so intense that subsequently two inches of the bone were removed. After this she recovered, and remained well until the year 1854, when she felt as though the limb was entire—as if the blood was rushing to every part below the amputation, accompanied by great pain in the nerves. The pain gradually increased until it became so severe and unbearable that she begged me to amputate the leg higher up. Upon her admission (Nov. 1858) I found that the cicatrix at one point was tied down to the end of the bone by a dense band about three-quarters of an inch long, and that any pressure upon this part increased her sufferings to a great degree. The end of the nerve, enlarged into a considerable bulb, could easily be distinguished attached by this band to the bone also. I had in other cases dissected out these bulbs, but with so little success that I was led to doubt very much whether the suffering was so dependent upon this cause as is usually supposed; whilst the result of the case just related induced me to hope that were the cicatrix released from the bone so as to permit of free movement, the patient would be relieved from her sufferings and spared a second amputation. I accordingly detached the cicatrix from the bone by a subcutaneous incision; the connecting tissue being dense, like cartilage. The soft parts were moved gently over the bone for a short time every day until the wound was healed and all trace of tenderness had ceased. The result was most satisfactory, and she left the hospital perfectly cured two months afterwards.

The alteration in the stump was certainly surprising. Prior to the operation the bone covered by the cicatrix projected nearly half an inch, the surrounding skin being flabby and puckered-in around it, and apparently devoid of fat; in point of fact, a conical stump was in process of formation. But when the patient left the hospital all signs of this misfortune had vanished, and the integuments appeared to have again become furnished with fat as in other parts of the body. The face of the stump became movable, soft, and full, and so thoroughly covered the end of the bone as not only to afford a good cushion, but entirely to dissipate all anxiety as to its projection. In proposing this subcutaneous section of the cicatrix I was prepared for the subsidence of pain, but I certainly did not anticipate the great improvement in the development and shape of the stump; and it may become a question of some interest, and one worthy of consideration, how far the adhesion of the cicatrix, and consequent drawing-up of the neighbouring skin to the bone and periosteum, may conduce to the formation of a conical stump, so usually attributed to insufficiency of soft parts and undue muscular retraction.—*Lancet*, *Sept.* 7, 1867, *p.* 288.

50.—SUB-PERIOSTEAL RESECTION OF THE OS CALCIS.

By Thomas Annandale, Esq., F.R.S.E., Lecturer on Surgery Assistant-Surgeon, Royal Infirmary, Edinburgh.

The following case is an illustration of the advantages of saving the periosteum in the re-section of bones.

The method adopted in this case of separating the periosteum and soft parts together, appears to me to be the most useful in such operations. (1st) Because the bone to be removed can in this way be thoroughly exposed; (2nd) Because the blood vessels of the reflected periosteum, and flap or flaps of soft textures, are not in any way injured, except along the line of the incisions.

For these reasons, I prefer this plan of operating to that in which the diseased bone is scraped away or gouged from the periosteum.

In separating the periosteum from the bone, it is of great consequence to save as much as possible of its inner layer, as the experiments and researches of Ollier have shown that this is the chief bone-producing portion.

During the temporary absence of Dr. Gillespie in June, 1865, J. F., aged 17, came under my care in the Edinburgh Royal Infirmary. The patient, a well-grown lad for his age, had suffered from a swelling of his heel for about a year before his admission, suppuration had taken place, and a sinus had in consequence formed over the back part of the os calcis.

On admission, the whole heel was swollen, and very tender to the touch, the os calcis felt enlarged, and there was a sinus immediately above the insertion of the tendo Achillis into this bone. On introducing a probe into the sinus it passed into a carious cavity in the substance of the os calcis. The other bones of the foot were quite healthy. Shortly after his admission the patient's sinus was attacked with sloughing phagedena, which was then prevailing in the wards, and this led to a considerable destruction of the soft tissues around. In the beginning of August, the sinus, which had become converted into a sore nearly as large as a five-shilling piece, began to contract, and on the 14th of that month was reduced to the size of a florin.

At this date the disease of the os calcis was in much the same state as when the patient was admitted, and I therefore thought it time to remove the affected bone.

Operation.—An incision was first made along the outer aspect of the bone, extending from the sore behind to the articulation between the os calcis and cuboid in front. Two short incisions were also made at right angles to and at each end of the first incision, the anterior one of which corresponded to the direction of the joint between the os calcis and cuboid, and the posterior one to the course of the tendo Achillis.

The incisions were made quite down to the bone; and then, with one of Lagenbeck's instruments, I scraped off the periosteum, and separated it and the flap of soft textures together from the entire surface of the bone. By extending the anterior short incision for a little distance into the sole of the foot, the flap was readily turned downwards and inwards, so as to allow the bone to be detached from its different articular and tendinous connections. Very little bleeding took place, no artery required to be ligatured.

The flap was re-adjusted, and a gutta-percha splint applied to the anterior aspect of the leg and foot, so as to keep the parts at rest. Six or eight hours after the operation a little oozing took place, but was stayed by stuffing the cavity with lint.

The wound suppurated freely after the operation, but the cavity gradually contracted, and, at the end of two months, the parts were soundly healed.

Four months after the operation, the foot had a very natural appearance—and the patient was able to bear a little weight on it.

A mass of very firm tissue had taken the place of the os calcis, and although this tissue was not as yet osseous, I have no doubt, from my examinations of other sub-periosteal resections, that in time a considerable amount of new bone would be produced in this case. Shortly after the patient returned to his home in the far north, so that I have never had an opportunity of verifying this opinion.—*Glasgow Medical Journal, June*, 1867, *p.* 44.

51.—CLINICAL REMARKS ON CASES OF JOINT INFLAMMATION.

By Richard Barwell, Esq., Surgeon to Charing Cross Hospital.

During the month of March there presented themselves at the above hospital an unusually large proportion of acute and subacute joint inflammations, the larger number of these being rheumatic and arthritic. In pointing out this fact to his class a few weeks since, Mr. Barwell made the following remarks:—

"The large proportion of synovitic cases that we have seen to-day and for some days past cannot fail to have impressed you; and that peculiarity of the time must, I think, be ascribed to the inclement weather, and especially to damp combined with cold. We had no run upon joint cases during the dry frosts of January; the damp warmth of February brought forth crops of boils and carbuncles; and the cold, raw, wet of this month of March has produced a disproportionately large number of rheumatic and rheumatoid affections. I make these remarks because it does not appear to me that sufficient attention has ever been given to the meteorology of disease. No sort of practice gives so extensive a field for observation as a great out-patient department, and I have for years past observed fluctuations in the class of cases to attend diversities of weather.

"But now I wish to call your attention to some peculiarities in my treatment of different cases. You will have observed that after the violence of inflammatory symptoms is subdued and that little pain with a certain amount of swelling is left behind, I order in some, the smaller number of cases, applications of iodide and bicarbonate of potash; but in others, the larger number, I combine in different proportions iodide of potassium and iodide of lead; for I find that of all absorbent applications, the iodide of lead is probably the most potent in cases of strumous and rheumatic thickening, but it must always be used with care, especially as it acts upon the gouty diathesis as a direct poison.

"Dr. Garrod has described, in his work on Gout, 'the influence of lead as a predisposing cause of gout' (p. 281, *et seq.*) I find, however, an important converse to this view—viz., the influence of gout as predisposing to lead-poisoning. The first notice which I received of this fact was in private practice. A gentleman for months laid up with gout, and tired of being overdosed with colchicum, sent for me. I ordered, among other things, an application to the foot, consisting of two parts of iodide of potass and one of iodide of lead. Next day—*i.e.*, in twenty-four hours—he had symptoms of lead-poisoning, by no means slight, and the blue gum line well marked. This fortunately gave me a clue in another case I then had in hand, of a gentleman who always fell ill after residing some days in his town house. He was the only one of the family who suffered. These illnesses were all alike, and his stay in town was always terminated or closely followed by a fit of gout. Having been given the strong hint above mentioned, I thought I could trace somewhat obscure symptoms of lead-poisoning, and on examination the water was found to contain lead. Since the cisterns, &c., have been changed, this regular sequence of events has ceased.

"Now you will observe that if any signs of gout are detectable in your patient, you will avoid the use of lead, and you may substitute a drug which in these cases is more useful—viz., the bicarbonate of potash, which has a chemically solvent action on gouty chalk-stone. You may apply the materials in solution. Let lint dipped into the fluid be placed round the joint, and over this oil-silk; or, if your patient be in bed, put a thick layer of wadding over the wetted lint, and confine the whole with tolerable pressure by means of a roller. The heat will add to the rapidity of absorption, and you will find this mode of application very efficient."—*Lancet, May* 11, 1867, *p.* 565.

52.—NOTES OF A LECTURE ON DISEASES OF THE JOINTS.

By Holmes Coote, Esq., Surgeon to St. Bartholomew's Hospital.

In speaking of inflammatory diseases of the articulations, I wish it to be distinctly understood that there are but two structures which admit of primary inflammation—namely, the synovial membrane and the articular extremity of the head of the bone. Of the former we have a familiar example in the case of hydrops articuli; of the latter, in the case of ulceration of articular cartilage. But this latter term is falling into disuse, for the ulceration of the cartilage depends upon a morbid condition of the cancellous tissue of the bone, and is consequently a secondary affection. Hence this group of affections is divided into "synovitis" and "disease of the joint;" the former for the most part transitory and easily managed, the latter more obstinate and attended with severe pain. It may be readily surmised how tardy is convalescence in the second instance, for the part of the bone intended to support the weight of the body is no longer in a condition to perform that function; it is painful and tender, and between its free surface and the cartilage now loosened there is a layer of exquisitely sensitive granulations. Hence the limb almost insensibly assumes that position in which there is the least amount of pressure between the opposed surfaces. In the case of the knee the joint is always flexed. In the further progress of such a case we find the ligaments softened by secondary inflammation, and they become elongated, so as to admit of displacement of the bones. During the acute stage any attempt at extension adds greatly to the patient's sufferings, and cannot, indeed, be borne.

The disease may be said to go on until the bone has recovered its healthy state, but it unfortunately often happens that before that time has arrived the articular cartilage has been in great part, if not wholly, destroyed; and then we meet with those cases, so common in the hospital, where the patient recovers with a bent and deformed limb.

In former times a bent limb was often amputated as a useless and inconvenient appendage, but we have now two methods of correcting the deformity; one by forcible extension, the patient being rendered insensible by the use of chloroform; the second, a painless proceeding by means of gradual extension, and although cases occur in which one proceeding may be demanded in preference to the other, yet I prefer, as a rule, the gradual extension. You cannot form any estimate of the amount of laceration of internal parts when force is used, and structures may be damaged which you wish to preserve.

Those cases which do not yield to forcible extension are cases of bony anchylosis—a somewhat rare occurrence—and you may generally ascertain whether the obstacle be bony or not by pulling at the limb. In the case of osseous anchylosis, the parts being immovably fixed, there is no muscular contraction in the muscular structures around. In the case of fibrous anchylosis, the more common form, there is some amount of yielding, and the surrounding tendons are felt to spring from their sheaths on the application of extending force.

In the event of the tendons opposing any serious amount of resistance they may be readily divided subcutaneously; but this is a measure rarely necessary in the young.

You are aware that I am no advocate for the resection of diseased joints. The successful results which are brought about by soothing remedies, rest, and by patience, have been amply illustrated by cases which you may see in the hospital.

We cannot always arrest morbid action; but we may keep the limb in that position which is easiest to the patient, and thus favours recovery. The restoration of the limb to its proper form is a matter of later consideration.—*Medical Press and Circular, April* 17, 1867, *p.* 358.

53.—TREATMENT OF DISEASED JOINTS BY ESCHAROTICS.

By Dr. Frederick Kirkpatrick.

[In the following paper, which was read at the last Annual Meeting of the British Medical Association, held in Dublin, the treatment recommended in cases of diseased bone and joints is incision, and the deep introduction of caustic, (the potassa c calce,) into the cancellated structure of the articulating extremity of the bone in the incipient stage, or that of inflammatory congestion, and into the joint itself in the very advanced periods.]

The statistics of conservatory surgery, more particularly of the great revived operation of excision of the knee-joint, are the reverse of satisfactory; the high expectations formed from the successful cases have not been fulfilled by a sufficient percentage of cures, and in many instances the reported cures have not been permanent.

In the last work on joint diseases, published this year by Mr. Holmes Coote, of Bartholomew's Hospital, the following conclusions are arrived at, based on the statistics of Dr. Hodges, of Boston:—"Considering the mortality after the operation, excision for hip disease does not merit a very favourable verdict. Excision at the knee, although occasionally yielding brilliant results, is an operation to be practised with great reserve. Excisions at the wrist-joint being followed by a large proportion of failures, and when successful, the usefulness of the hand being so limited, are operations not sanctioned by sound judgment or conservative surgery. Operations on the foot for strumous disease, usually yield unsatisfactory results."

Dr. Hodges, of Boston, in his "Essay on the Excision of Joints," observes:—"Out of a considerable number of cases, one-third died, and more than one-third are known to have failed." I repeat then that a period has arrived when doubt and uncertainty pervade this most important department of surgery, and it has become a qustion as to whether there was not a greater saving of human life, when early amputation was the rule, than in the interval that has since passed over. It is also especially worthy of remark, that excision has rarely, if ever, been performed in private practice.

At the commencement of his magnificent address at Chester, Mr. Bowman, that great benefactor of his race, called surgery "the hands of God, the human hands." I may be permitted to add, hands never to be put forth to the execution of any operation, but when their possessor can say he would himself consent to its performance were he the patient. I believe that the rigid application of this golden rule in surgical ethics, would very much limit the future performance of operations for excisions, save only at the elbow-joint.

In the surgical charge of the North Dublin Union, the largest chronic hospital with one exception in this country, I have had, during the last twenty-five years, ample opportunity of judging of the effects of rest in the treatment of those affections, and the result of my experience has been most unfavourable.

A certain amount of success may be looked for in the treatment of the upper classes, where the purest air, the best nourishment, the most approved mechanical appliances, together with the means of easy locomotion, can be commanded ; but, with the lower classes, I have found that the treatment by rest has been a history of failure ; disease spreading from bone to bone in the smaller articulations, and before the consolidation of a large joint could be completed, organic disease having, in general, invaded the liver, or some of the other internal organs.

This unsuccessfulness forced me some years ago into the discovery of this treatment by cauterization which I propose. Having often remarked the healthy reparative action that followed the use of the potassa c. calce in sinuses in the groin, neck, and axilla, I began to introduce it into fistula leading down to diseased bone, at first with caution, then more boldly, and finally disregarding Sir B. Brodie's strong injunctions against letting potassa fusa enter a sinus, I proceeded to carry its action deeply down, converting the small contracted painful orifices into large funnel-shaped openings, and bringing the carious bone into view, and within reach of the further application of the caustic. In this mannér, several cases of disease of the carpus and tarsus, and of the flat and superficial bones, were successfully treated, the caustic being reapplied at intervals of a few days, to keep the orifices freely open until the carious bone had disappeared, or was covered over with firm granulations.

In a similar manner, several cases of chronic necrosis were treated, the caustic being very freely used, destroying all foul undermined integument, and leaving, after the removal of the sloughs, large clean circular openings, more than an inch in diameter, and extending deeply down to the sequestrum, into contact with which the caustic, in stick and powder, was freely brought.

In this manner, two cases of necrosis of the fibula, very similar to each other, in which numerous openings led down to diseased bone, and where the patients were reduced to the lowest state by years of suffering, were perfectly cured within six months.

In a case of necrosis of the heel in a delicate lad, who was deformed by the effects of an old hip disease, a caustic perforation was made at each side of the heel, and the powder was brought into contact with the dead bone, until it was so removed that a catheter was passed quite through the heel, no inflammation or constitutional disturbance having been caused or excited.

I can speak with the utmost confidence of the application of this remedy in all such cases of caries affecting the superficial bones.

The caustic perforations may be multiplied in proportion to the extent of the disease, respect being paid to important nerves and vessels, and care being taken not to destroy sound structure, or periosteum beyond the limits of the diseased surface of the bone. Before I speak of the application of this remedy to the arrest of the early stage of joint disease, I may be permitted to allude to the important question as to the structure which is first attacked.

My own opinion accords with those authors who believe that in the great majority of cases the disease commences in the cancellous structure of the heads of the articulating bones. That there are rare cases where it begins in synovial membrane or cartilage, and mixed ones which, seen at a late period, may perplex diagnosis, I fully believe; but I consider that those instances where the fons et origo mali arises in the bone, preponderate so enormously that, as a rule of practice, it cannot be too strongly insisted upon. My own opportunities of obtaining pathological evidence have left me without a doubt on the subject, and the practice which I propose is based upon that conclusion.

In Sir B. Brodie's great work on the joints, he recognises the fact of the frequent origin of disease in the cancellous structure, and he describes the heads of the bones as distended, with a reddish medullary fluid, then softening of the tissue, and finally suppuration—the matter either forcing through to the cavity of the joint, or reaching the surface at some position more or less remote; but whilst he thus clearly recognises the cause, and graphically describes the disastrous effects, he forbids the remedy, and strongly cautions against an early or premature opening, although he had himself with success trephined the heads and shafts of the long bones in cases of painful and circumscribed abscess.

That he attempted to give relief in a similar manner in those cases of acute articular osteitis which he so circumstantially describes, is more than probable; and I therefore infer that his caution against the early opening of an inflamed bone, was founded on his experience of ill consequences that followed such a proceeding.

Notwithstanding the teaching of this great authority, I venture to propose interference by operation at the very earliest moment that congestive inflammation of the head of a bone can be fairly diagnosed; and I state with confidence that a perforation made into the cancellous structure, if freely cauterised with the patassa c. calce, will be followed with relief from pain, and that the inflammation which ensues will be only such as is attendant on and accompanies reparative action.

The caustic tunnel may be made at once by cutting down on the bone and piercing the compact tissue with a strong knife, trochar, or small trephine, and then freely cauterising the full extent of the perforation; or, in less acute cases, a small eschar may be first made, the centre of which being incised, the caustic can be introduced, and, by combining its action with the knife, the tunnel can be carried deeper, from day to day, in a gradual manner. By means of this combined caustic perforation, I succeeded in arresting disease in its first onset in the head of the radius in the case of a young man, aged twenty-four, in the year 1861. Since that time I have tried it with success in several cases of incipient disease, in carpal, tarsal, and other superficial bones.

I perforated the tibia above the internal malleolus in several instances with curative results, also the great trochanter, in cases where its structure, or that of the head and neck of the femur, was the seat of osteitis, the cavity of the joint being yet unaffected.

On the 26th of last April I exhibited a young man at the Surgical Society of Ireland, into whose trochanter a caustic tunnel had been inserted on the 22nd of March preceding, and which was still freely open. The wasted buttock, obliterated fold of nates, and emaciated state of the entire limb, still existed to proclaim the nature of the disease; but all pain had ceased, the motions of the joint were in a great measure restored, and he walked about before the members of the society without lameness, although for three months previous to the operation his thigh was flexed, he had constant pain in his hip and knee, and he could not admit abduction or any extended motion of the limb. This patient took his discharge to work on the 11th of May, the contour of the limb being almost restored to its natural fulness, and his general health quite recovered.

Although strongly recommending this treatment for the early stages of acute articular osteitis, and also for the very advanced periods when caries is established, or when a whole joint is converted into a foul suppurating cavity, my experience does not warrant me in advising the practice so strongly in the intermediate periods, where the head of a bone may be the seat of a diffused suppuration, possibly communicating with the joint itself. The caustic, I fear, in this condition of parts could not extend sufficiently to protect from the constitutional disturbance and risk of pyæmia that might follow.

The principal merit I claim for this caustic treatment is, that it is a powerful, and, at the same time, the safest means of correcting nature where she is manifestly in error, and of assisting her operations where they are directed aright. And here, I have the boldness to start from out the well-worn professional groove of bestowing a blind admiration upon the proceedings of nature, as seen in her efforts to restore and repair diseases of bone. Instead of the vis mediatrix, which is so perfect, and to be relied on in many of our ills, I assert that she exercises in those affections a vis inimica, a vis perniciosa.

From the first onset of inflammation within the cancellous structure, all through the various destructive processes that ensue, she is engaged in hemming up, and confining the engaged bone, and perpetuating its diseased state, and her reparative operations only commence and become effective when, either by man's art, or by her own late remorseful ulcerations, this imprisonment at the surface is interfered with, and in a greater or less degree removed.

The highest art then of the surgeon is demanded to counteract and remove her mischievous operations from without, and to hasten, open up, and make way for her all powerful reparative actions from within. I am fully convinced that this interference can be more safely and effectually attempted by the combined action of the knife with cauterization, than by the knife alone, and I commend the treatment to my professional brethren with the greatest confidence, earnestly hoping that they will give it a fair trial, and that it may be attended by the same measure of success in their practice, that it has been in my own.—*Medical Press and Circular, Aug.* 21, 1867, *p.* 172.

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54. POWER OF ABSORPTION OF WOUNDS AND ABSCESSES.

M. Demarquay has submitted to the Academy of Medicine of Paris a series of experiments on this subject. He placed upon wounds and into accidental cavities aqueous solutions of iodide of potassium of the strength of 10 per cent. After from six to thirty minutes iodine was found in the urine, and especially in the saliva. The author considers that ulcers and abscesses absorb also noxious gases both from the atmosphere and those formed by the decomposition of blood and pus. He, therefore, advocates the protection of wounds and abscesses from the surrounding air, dressing them with glycerine, alcohol, or disinfectants; and surrounding patients with as pure an atmosphere as possible.—*Lancet, Sept.* 21, 1867, *p.* 360.

55.—NEW MODE OF TREATING COMPLICATED FRACTURE OF THE LOWER JAW.

Case under the care of C. G. WHEELHOUSE, Esq., at the Leeds General Infirmary.

[In the following case some special mode of treatment was evidently called for. There was a severe contused and lacerated wound of the left cheek, communicating directly with a fracture of the lower jaw, and extending upwards on the face for two inches and a half or three inches. The jaw had sustained three fractures : one at the symphysis, a second immediately in front of the insertion of the masseter muscle, and a third through the ramus of the bone and base of the coronoid process.]

Two silver pins were made, with flat, circular, and *perforated* heads, each pin being about an inch and a quarter in length. Two holes were bored with an Archimidean drill through the substance of the jaw-bone—one between the roots of the outer incisor and canine teeth of the unbroken side, and the second between the roots of the same teeth of the fractured side. Through these holes the two pins were passed *from behind forward*, the perforated heads, threaded with a good stout silk ligature, resting upon the floor of the mouth under cover of the frænum of the tongue. Having been well thrust forward through the drill-holes, the points were bent in opposite directions, the loose fragment was placed in good position, the ligature was brought forward over the teeth, and a figure-of-8 suture was then made round the reversed ends of the pins.

By this means perfect apposition was secured. The wound in the soft tissues of the face was allowed to granulate untrammeled by external pressure. The patient was enabled to take food easily and well, and, in short, made so excellent a recovery that he left the hospital on the 26th of November, exactly a month from the date of his admission, with his wounds all healed, his jaw moderately firm, and the line of his teeth perfect.—*Lancet, Aug.* 17, 1867, *p.* 195.

56.—ON THE RAPIDITY OF THE PULSE WITH REFERENCE TO RISKS OF OPERATIONS.

By JAMES PAGET, Esq., F.R.S.

[The following short paragraph we take from a lecture "On the Various Risks of Operations."]

People with slow pulses bear operations just as well as those who, in all other respects than that of their heart's action, are like them. And people with habitually rapid pulses are not bad patients, if the rapidity of the pulse be not associated with some organic disease. Especially, you will find a considerable number of children and young persons, chiefly sensitive girls, whose pulses are rapid enough to frighten you. Observe whether the respirations are in the same proportion rapid; if they are not, the respirations, and not the pulse, must be your guide in judging what is the patient's state. Many a time I have pointed out to you a pulse beating 120 or 140 times in a minute, and said that it meant no mischief, because the respirations were not more than 20 or 25.

And there is a set of cases in which you must always apply this rule of checking the indications of the pulse with those of the breathing—namely, cases of hemorrhage. After large bleedings, when the patient recovers from their immediate effects, the pulse is usually hastened, and the breathing is retarded; so that with a pulse of 120 or more there may be not more than 10 respirations in the minute.

Mere irregularity of the pulse, if it be habitual, and not connected with valvular disease or degeneration of the heart, does not, so far as I know, affect the chances of recovery from operations. If the structure of the heart, as well as its functions, be disordered, you must judge according to such rules as I have just stated.—*Lancet, Aug.* 10, 1867, *p.* 152.

57.—ON THE REDUCTION OF DISLOCATIONS OF THE HIP-JOINT BY MANIPULATION.

By THOMAS ANNANDALE, Esq., F.R.S.E., Assistant-Surgeon, Royal Infirmary, Edinburgh.

[The method of reducing dislocations of the hip-joint by simple manipulation is by no means modern. It was described by Hippocrates, Wiseman, and other early writers. The method is so simple and successful in the majority of cases—so much more easy in its accomplishment than the plan in which the pulleys are used, that in time its use will probably entirely supersede the latter. In the following case the patient was a strong muscular man, about 48 years of age. The head of the femur was dislocated upwards upon the dorsum ilii.]

The patient was placed on a mattress laid on the floor, and then put completely under the influence of chloroform until the muscular system was thoroughly relaxed. The pelvis being steadied, I seized the left ankle with my right hand, at the same time grasping the corresponding knee with my left hand, and flexed the leg to its complete extent upon the thigh, and the thigh upon the abdomen. With the limb in this position, the knee and foot were made to take a circular sweep outwards as far as they would go, and then the whole limb was suddenly extended. This movement caused the head of the bone to slip into the acetabulum at once with a distinct jerk.

In some of the dislocations of the hip-joint the direction of the circular sweep should be made inwards, and a little rotation of the limb often assists the head of the bone into its place.

Occasionally a recent dislocation of the hip-joint can be reduced by simple extension and manipulation, without the employment of the pulleys. The next case was treated successfully in this way:—

A miner, aged 48, having sustained a dislocation of the hip-joint backwards into the ischiatic notch, was admitted into the Clinical Surgical wards of the Royal Infirmary in March 1866.

The patient having been placed on a mattress, was put fully under the influence of chloroform, and then by slight extension and manipulation of the limb, Mr. Syme readily turned the head of the bone into the acetabulum.

If the head of the bone should have become fixed in a recent or old-standing dislocation, and cannot be moved by flexion and manipulation, it will, I think, be found useful to make some extension in the proper direction, either with or without the pulleys, until the head of the bone is dislodged, and then, suddenly removing the extending force, to adopt the flexion and manipulation method.

In this way I lately easily reduced the following case of dislocation of the hip-joint upwards on to the pubes.

A man, aged 62, was admitted into the Clinical Surgical wards of the Royal Infirmary on the 2nd of March 1867, with a dislocation of the hip-joint on to the pubes, of three days' standing. Having placed the patient on a mattress, and put him completely under the influence of chloroform, the flexion method was tried, but the head of the bone would not move from its abnormal situation. Extension of the limb in a direction outwards was then made by means of the pulleys, and as soon as the head of the bone could be felt to leave the pubes, the extension was suddenly withdrawn, and at the same moment the flexion plan was adopted, the foot and knee being also rotated inwards. As soon as this was done, the head of the bone slipped into the acetabulum.

As an example of what may be done by manipulation in reducing a dislocation of another joint, let me relate a case which was sent to me a few weeks ago.

A big strong man, aged 50, came from the country, in February last, with a dislocation backwards of the first phalanx of his left thumb. Careful and repeated attempts had been made to reduce the dislocation in the country, but they had proved unsuccessful. Having chloroformed the patient, I first tried extreme flexion, and afterwards extreme extension (in a direction backwards) of the thumb, but without succeeding in reducing the bone. It was then my intention to have divided subcutaneously the agents preventing reduction; but before doing so, I grasped the thumb, slightly flexed it, and after a few gentle rotations and manipulations, I was rejoiced to find that reduction had been accomplished, and therefore my operation was not required.—*Edinburgh Medical Journal, May* 1867, *p.* 997.

58.—ON MAXILLARY DISLOCATION AND ITS REDUCTION.

By Dillon Kelly, Esq., Medical Officer of the Milltown Dispensary, Co. Westmeath.

If we study the physiology of the muscles of the lower maxillary articulations, we shall find that the external pterygoids, in consequence of the direction of their fibres being nearly at right angles with that of the temporals, masseters, and internal pterygoids, must, as a consequence, act at nearly right angles with the action of those latter muscles, they also being the principal muscles in carrying the lower jaw forward; that being assisted, to a certain extent, in that office by the superficial portions of the masseters and internal pterygoids, those being the proper grinding muscles, their actions must, as a thing of course, be associated with the motions of those muscles, and act simultaneously with them. Now, Mr. J. P. Vincent has shown very clearly that as almost all muscular actions are performed in reference to a centre, so when by any accident that centre is deranged, if an abnormal or a new centre can be readily formed, a new combination of muscular action is at once developed, as conformable as may be with the centre produced. When dislocation of the jaw takes place, a new centre is formed; and not only, therefore, does the direction of the muscles involved in that dislocation become altered, but their lengths from their new positions become altered also. Thus the external pterygoids become considerably shortened, in consequence of the new positions of the condyles anterior to the transverse roots of the zygomas; whereas the internal must, to a certain extent, become elongated by the throwing backwards of the angles of the maxilla.

Now, admitting that the muscles adapt themselves to their new positions and centres, and that the external pterygoids must be considerably shorter in maxillary dislocations than in their normal states, it follows that a degree of spasm of these muscles must be present to account for their abnormal decurtation.

Assuming, therefore, that the internal pterygoids become elongated from the elevation and recession of the angles of the inferior maxilla, they, from an opposite cause, must be the subjects of spasmodic action also.

Here, then, we have two causes, both conducing to the same end, yet either of them, quite sufficient by itself to produce all the phenomena of muscular spasm.

If, then, we admit that the centre of motion of any articulation being accidentally changed, the muscles of that articulation will, in consequence of the irritation assumed by all muscles, when they are forcibly thrown out of their line of action, take on or exert abnormal action of extraordinary power. We have, then, another cause sufficiently powerful not only to account for all the phenomena of maxillary dislocations, but also capable of opposing an almost insurmountable obstacle to their reduction. A moment's reflection on the appearances presented by such a dislocation must at once convince us that such an alteration has taken place. Continuing our examination, it will become evident that the external pterygoids, acting under the influence of the pathological law already enunciated, draw the condyles forward with almost irresistible force towards the pterygoid processes of the sphenoid, and hitch them against the transverse roots of the zygomas, which thus become their *points d'appui*, and produce a sort of dove-tailing between the depressions on their necks and the ridges constituting those roots. That the remaining elevators of the articulation are not in a state of quiescence, may be inferred when we take into consideration the direction those muscles assume in the new arrangement of the centre of motion, the internal pterygoids drawing the maxilla upwards and forwards, whilst the masseters and temporals draw it upwards and backwards. Here, then, we have, in like manner, another combination of forces, almost diametrically opposed to each other, yet all tending to the same end, and requiring, I think, no great stretch of the imagination to presume that the increase of muscular action developed in consequence must be the chief obstacle to the reduction of such a dislocation.

To M. Nelaton, however, is the merit due of being the first to act on the inference derived from what he supposed the chief obstacle to reduction, and of practically applying means based on that inference, by directing the patient to open his mouth as wide as possible when about to reduce a maxillary dislocation; at the same time, that he completely overlooked the equally palpable inference to be drawn from his first step in the process of reduction, namely, the relaxation of all the muscles engaged in the dislocation by the action of opening the mouth.

Mr. Vincent, in his work on associated muscular action already referred to, observes:—

"That the overpowering strength which a muscle is brought to exert when its usual direction of action, about a centre, is forcibly changed, is another law of conditions of the utmost consequence to the surgeon.

"The dislocation of the patella on its edge is an example; the bone sets the extensors of the leg into action of the most violent kind, these muscles act most powerfully when the limb is to be straightened, and, in the condition in question, they act with a power that defies all the force that human aid can call to its service.

"Their force is also partly the effect of that irritation which all muscles get when they are thrown out of their ordinary line of action, particularly when they are disturbed in moving round their ordinary centre of motion. We have only to flex the leg a little, and all this powerful opposition to restoring the patella ceases on the slightest rotatory motion.

"The disturbed arrangement here, was the elevation of the centre of action of the extensors above the ordinary position, and as these muscles, in the straight position of the whole limb, are called upon to support a great proportion of the weight of the body, so when in that position, they are naturally impelled to exert vast force.

"But, in obedience to the associated action of combined muscles, when the leg is bent, and another order of motions in this complicated joint, brought into play—then, these extensor muscles immediately relax, as they would otherwise, by their action, prevent the rotatory motion of the leg upon its axes; thus, the moment the leg was bent, the extensors returned into a comparative state of repose and left the patella quietly to resume its appointed position."

Acting on that principle, in May, 1839, I reduced, with the greatest ease, a maxillary dislocation produced by the retching of gestation in a stout young woman, the wife of a respectable trader in this town, to whom the same accident occurred repeatedly afterwards.

In March, 1847, Mary Thompson, aged 55, a beggar-woman, of stout make, dislocated her lower jaw by falling against the end of her bed at night.

Her neighbours, in the morning, seeing she could not shut her mouth, unanimously were of opinion that she was "fairy-struck." Firmly believing in the truth of this superstition, she sought no surgical aid, and remained in a state of suffering for four days; yet at the same time making every effort to procure funds to bring her to a fairy practitioner at some distance.

Whilst thus engaged, she accidentally called on Mrs. F., for some relief, who at once recognized the nature of the accident, and sent her to me.

In this case also, notwithstanding the length of time that intervened, and the muscularity of the patient, I reduced the dislocation without the slightest difficulty.

Lastly, in August, 1850, J. M'Donnell, aged about 30, tall and rather muscular, having gone to the funeral of a friend, and drank too freely, the violent retching that succeeded dislocated his lower jaw. Being up to that period a teetotaller, and not knowing what had occurred, or the nature of the accident, he and his family imagined he had suffered an infliction for breaking his pledge, from which he was destined never to recover; and I found him, in consequence, although still labouring under the effect of intoxication, in a state of the greatest distraction.

In this case too I was equally and instantaneously successful.

In accordance with the theory, therefore, of associated muscular action it is necessary, for the reduction of this dislocation, that the pterygoids, masseters, and temporals should be relaxed, as being the chief or opposing forces to its reduction. And to relax them, it is necessary that the digastricus, mylo-hyoid, stylo-hyoid, and genio-hyoid, or group forming the depressors of the lower jaw, should be called into action by opening the mouth.

To reduce it, therefore, place the balls of the thumbs, covered with a handkerchief, on the crowns of the posterior molars, grasping the angles of the jaw externally with the fingers; then desire the patient to open the mouth, when the slightest pressure downwards, during the act of opening it, will be sufficient to disengage the necks of the condyles from the transverse roots of the zygomas, and will permit the temporals and masseters to at once reduce the dislocation by the reinduction of their normal action.

In conclusion, so easily is such a dislocation reduced in some subjects by that mode of treatment, that I instructed Mrs. F. in the manipulation, who was quite familar with the accident, and who also became so familiar with the principle, that she was accustomed, to reduce her own dislocations by pressure with her indices placed on the crowns of the posterior molars.—*Dublin Quarterly Journal, Aug.* 1867, *p.* 36.

ORGANS OF CIRCULATION.

59.—ON THE RAPID PRESSURE TREATMENT OF ANEURISM.

By Dr. W. Murray, Newcastle-on-Tyne.

The object of this paper is to prove, first, that aneurisms of the largest size can be treated successfully by a process which is so rapid in its operation as to occupy less than an hour; and, second, that the cure takes place in these cases by coagulation of blood in the sac of the aneurism, and not, as has been hitherto believed, by the deposition of fibrine.

What is the nature of this treatment? The patient is put fully under the influence of chloroform, that we may be enabled to apply a very powerful pressing instrument on tender or sensitive parts, such as the site of the abdominal aorta. The full administration of chloroform is further necessary to relax the muscular system, which is an important condition of success, as the slightest movement of the pressing instrument by the muscular action of the parts pressed on, is fatal to this process of cure. I would draw special attention to the next part of the treatment, as success is dependent upon the care with which this is carried out. *It is the complete arrest of all movement of the blood in the aneurismal sac.* By the old method of cure, fibrine was supposed to coagulate and be deposited from the very slow, scanty, and feeble current of blood which found its way through the sac of the aneurism while incomplete pressure was maintained. Such a current, however feeble and slow, would be absolutely fatal to the rapid treatment. Complete arrest, therefore, of a current of blood, which shall be retained in a motionless state, is the secret of success. In fact, you must do by pressure exactly what is done by the ligature when it is applied to the artery above the aneurism; and you may do even more than is done by the ligature; for Dr. O'Ferrall, of Dublin, has insisted upon the application of distal as well as proximal pressure; and Dr. Mapother has carried out his suggestion with complete success. Aneurisms requiring pressure on the abdominal aorta are perhaps least dependent on distal pressure, as the collateral circulation to the lower parts of the body is here so limited as to render a current into the distal orifice of the aneurism improbable. In order to obtain this complete arrest of blood in the sac of the aneurism, the most careful and energetic watching of the pressing instrument is necessary. You must have so deep an interest in the treatment that you will sit for a whole hour or more, enduring the most trying strain on your muscular and nervous systems, before you can hope to obtain a cure. Your eye and hand must be continually testing the condition of the aneurismal swelling; and the faintest indication of pulsation there must be considered fatal to the process of cure, and at one remedied; in fact, the tourniquet must be so placed as not to permit a single rush of blood into the aneurism. Nothing short of this will bring about a cure in a short space of time.

We must now consider the duration of the treatment. At Newcastle, a case of aneurism of the abdominal aorta underwent the process of cure in three-quarters of an hour; and, in another case at Sunderland, under Dr. Heath's care, consolidation was distinctly observed to occur within twenty minutes. In the Newcastle case, unsuccessful efforts had been made for four hours; and, at the end of that time, the aneurism remained unchanged. As neither increase of solidity, diminished pulsation, nor decrease in size, could be detected, I determined to make a final effort. The patient being fully under the influence of chloroform, I re-applied the tourniquet, and held it firmly and securely over the aorta so as to obliterate every trace of pulsation. By a prolonged effort, three-fourths of an hour passed without a single slip of the instrument. It was then removed, and the aneurism had ceased to pulsate. A slight movement was perceptible for some time afterwards; this being an impulse communicated from the pulsation of the aorta above it.

In Dr. Heath's case, the pressure had been kept up irregularly for about ten hours, when the patient fainted under the chloroform. The pressure was then removed, and the pulsation and other characters of the aneurism were found to be as bad as ever. The patient was then urged to bear a final attempt without chloroform. This he did; and, to our amazement, when, at the end of twenty minutes, he declared he could bear the pressure no longer, we found the aneurism had become solid and had ceased to beat.

Here, then, are two cases in which the actual process of cure was brought about in less than an hour. How is this effected? We affirm by coagulation of blood in the sac of the aneurism. We believe this, because of the short time occupied in bringing about a sudden and decided change in the disease. It seems to me impossible that fibrine can be so rapidly deposited from the blood in such quantities as to fill up the cavity of a large aneurism, and to change a pulsating, thrilling, and expanding sac into a motionless solid mass. This opinion is supported by another important fact—the large soft mass which is produced by the filling of the aneurism with solid matter disappears in a few hours. In each of the above cases, all trace of the aneurismal tumour was gone before we could obtain a plaster cast of the part. Rapidity of disappearance, then, as well as rapidity of production, is in favour of the existence of coagulation of the blood. In it we have contraction of a clot of blood inside the aneurism, by which the watery parts of the blood are squeezed out and absorbed, such as occurs in a clot of blood outside the body.

In conclusion, we may observe that the solution of fibrine in liquor sanguinis is, like all complicated natural phenomena, to be regarded as resulting from the adjustment of certain conditions to the forces by which it is maintained. When we disturb the conditions, the phenomenon ceases, to be replaced by another equally remarkable—coagulation. In fact, by substituting the conditions of coagulation for those of the solution of fibrine we cure aneurism. The day may come when even a simpler plan than pressure will effect this change ; but, at present, it must be admitted that a great advance has been made in limiting the time and altering the process by which this disease may be cured.—*British Medical Journal, Oct.* 5, 1867, *p.* 287.

60.—ON THE TREATMENT OF ANEURISM BY COMPLETELY ARRESTING THE CURRENT THROUGH THE SAC.

By Dr. E. D. Mapother, Surgeon to St. Vincent's Hospital, Dublin.

The treatment of external aneurisms by compression, one of the greatest of modern surgical improvements, was established by Dublin surgeons. They advocated such a degree of compression as would send the blood gently flowing through the sac : layer after layer of coagula being expected to form, as happens, it was asserted, when aneurisms cure spontaneously. Arrest of current, and clotting of the blood in the sac, were deprecated ; but it seems to me that the following cases show that these events are, on the contrary, most desirable.

Case 1.—J. D., aged 25, healthy, was admitted into St. Vincent's Hospital, January 14th, 1865, for right ilio-femoral aneurism, which had begun without injury five months before. Digital and partial instrumental pressure having failed, I tried to stop the common iliac with an elastic compressor, the patient being kept under chloroform for twelve hours. No clot formed. An anthracoid slough formed at the point of pressure.

Five days afterwards, another attempt was made, after the following preparatory steps. The abdomen was made lank by emptying the bowels and bladder ; the limb was raised, bandaged and fixed to aid venous return, and to render increased flow of blood for muscular action unnecessary ; and the sac was compressed by an elastic roller, so as to contract the space to be filled by the clot as much as possible. At Dr. O'Ferrall's suggestion, the superficial femoral was stopped, so as to keep the sac full. Signoroni's clamp was then fixed over the common iliac artery for four and a half hours, when the tumour was found solid and pulseless, the common and external iliacs being still pervious. Absorption and complete cure followed.

Case 2.—J. B., aged 35, healthy, was admitted into St. Vincent's Hospital, May 1866, for left popliteal aneurism, which followed a strain a fortnight before, while getting down from his cab. The sac was as large as a turkey-egg, and towards the biceps appeared diffuse. Digital and elastic instrumental pressure failing, stoppage as complete as could be achieved in a very restless patient was kept up for five hours, when the sac was found pulseless. Thirty-six hours afterwards, pulsation recommenced. After three other attempts, unsuccessful because chloroform was refused, the femoral at Scarpa's triangle was compressed, and the flow of blood out of the sac was impeded by tight bandaging and elevation of the leg, distal pressure on the popliteal not being possible. The patient was kept apathetic, not insensible, with chloroform, for nine and a half hours, when the sac was found hard and pulseless. At the compressed point, a superficial slough formed; but, with this exception, the recovery was rapid and perfect.

In both cases, bromide of potassium internally, and ice locally, gave aid by lowering the circulatory force. A good meat diet was given, to increase the plasticity of the blood; but drink was not restricted, for it is unphysiological to suppose that thus we can thicken that fluid. Fresh vegetable food augments the plasticity of the blood in scurvy, and may be reasonably allowed in the diet of a patient with aneurism.

Pain has been the great obstructive to partial compression; and complete pressure would be unendurable without chloroform, that inestimable boon, which we owe to the genius of Simpson. By acupressure also, that philosopher has enabled us to treat cases such as the innominate aneurism now under Mr. Porter's care, in which compression was inapplicable, and ligature would be mortal. Chloroform darkens blood but does not lessen its coagulability.

Distal pressure should precede and accompany proximal pressure; for thus the sac is kept full of blood at rest, and the resulting clot will equal its cavity. During the many hours which elapse before the separation of clot and serum is complete, the sac will probably contract on the clot, while the serum oozes away or is absorbed. In popliteal or antecubital cases, distal pressure being impracticable, Mr. Hart's flexion plan may achieve the same object. I doubt that aneurism has ever been cured by such pressure as would only lessen the calibre of the artery leading to the sac; for then the flow of blood being quickened, and the efferent vessel being larger, the blood would not remain long enough to clot. The blood flowing in would carry the agent to which the fluidity of fibrine is due; but, if the blood in the sac be isolated, this volatile agent may permeate its coats. In cases said to be cured by partial compression, it is probable that, the pressure having been increased to the amount of complete arrest for a short time, clotting in the sac, or plugging of the artery below it, produced the cure; for the pulsation is usually reported to have suddenly ceased.

Digital pressure has of late been most successful, because it is usually employed to the degree of complete arrest. The evidence to prove that, in cases cured by partial compression, the sac is filled by laminated fibrine, is most insufficient; and, in fusiform aneurisms or sacculated aneurisms freely opening into the ruptured artery, it is impossible to conceive such an occurrence. In the fifty-three cases cured by compression, and detailed in the *Nouveau Dictionnaire de Médecine et de Chirurgie*, nine solidified under twelve hours, and four under four hours, in which time this much talked of stratification could not have been accomplished.

A few hours (perhaps less than an hour, as in Dr. Murray's aortic case) suffice to clot the blood isolated in the sac; while cure by partial compression takes, on the average, twenty-five days. One believer in the method persevered in its use for nine months. The anxiety and confinement probably lessen the coagulability of the blood; and it has only succeeded in 62 per cent. of the cases in which it was tried. Complete pressure has succeeded in at least four cases; and I am not aware that it has ever failed. With two pressing points, and as soon as experience reduces the time of pressure to a minimum, sloughing need not be feared. In conclusion, I claim for this method greater rapidity and greater certainty than partial compression, and far greater safety than deligation.—*British Medical Journal, Oct. 5, 1867, p. 286.*

61.—ON A RARE CASE OF SIMULATED ANEURISM.

By Thomas Nunneley, Esq., Surgeon to the General Infirmary at Leeds.

[The following is, if not an unique case in surgery, at least one of the most curious and rare ever seen. There was a comminuted fracture of the neck of the thigh-bone, of the occurrence of which no history was obtained, either from the patient, his wife, or his sons. It must have occurred without violence and almost without symptoms, so that it was never even suspected by anyone. The abnormal distribution of vessels was most unusual, and, in connexion with the unknown fracture, simulated in a very exact manner a disease which had no real existence.]

You will therefore perceive that, in the outset, I openly confess to a mistake in the diagnosis of this case. Now, in so doing, I am not about to excuse myself by saying that if I erred I did so in good company, nor to shield myself behind the fact that several others agreed with me in the opinion which I formed; for no surgeon should shrink from the responsibility which attaches to himself individually. But I am about to say, now that a review of all the facts may be made, I do not consider that either I or they were to blame; as I do not think any other opinion could *then* have been formed, though perhaps *now*, and for the future, what we have learned by this case may be of use. At any rate it affords another illustration of how carefully we ought to investigate every symptom and explanation which by possibility may illustrate any case, and not to rely too implicitly upon those appearances which are even stated to be pathognomonic. It may even, perhaps, dispose some of you to incline to the conclusion that, with all the care you can exercise, you may occasionally be unable to avoid error; though, let me tell you, my object would much rather be, when you have committed a mistake, to stimulate you by increased study to render a repetition of the mistake impossible, than to induce a comfortable feeling of an unavoidableness and non-responsibility which would make you sit down contented under it. No; if such a feeling were to take possession of your minds, errors would soon cease to be mistakes—they would become crimes.

I will relate the case, and then add such comments upon it as I think may be useful to you, when you will see how these remarks are borne out by the case itself.

Albert H., aged 44, coal-miner, working at the Oaks Colliery, near Barnsley, presented himself on Friday, Nov. 16th, 1866, for admission into the Leeds General Infirmary. He had walked from the railway station to the Infirmary. He happened to be the last patient examined, and, as it had been a heavy day, he had waited in the room at least three hours. He walked up to the consultation table without any difficulty, into the private room where he stripped himself for examination, back to the table, and then up to his ward on the upper floor, without assistance. He had the look of a suffering man. Though somewhat lame of the left leg, and using a stick, there was neither eversion nor inversion, nor shortening of the limb. The body was bent somewhat forward, but the whole foot was placed on the ground in walking. The pain and stiffness of which he complained he referred to the groin, and not to the hip. On this side (the left) there was a swelling just under Poupart's ligament, in the course of the femoral artery, which was perceptible to both sight and touch. There was decided pulsation. The tumour was not large, but was well defined, and certainly under the fascia lata. He stated that he had never had any accident or injury whatever, nor did he recollect having at any time had a fall; but that twenty-two weeks before, while in perfect health, as he was walking on level ground to his work as usual, he suddenly felt something give way in his groin. He immediately felt sick, and was unable to walk. The pain was so intense that he became helpless, and had to be assisted home by two fellow-workmen, between whom he walked a considerable distance. Since then he has not been able to do any kind of work, or move the leg much. The suffering has principally been in the groin. There is neither deformity nor tenderness about the buttock.

This account of the commencement of the attack he repeated many times in my hearing, and also gave it in precisely the same terms to all the surgeons, all the officials, the dresser, and to other students,—for, as it was at once pointed out as an interesting and important case, many of them took notes of it. I dwell upon this because the statement formed an important element in the diagnosis. I may here also mention that, during the few days he remained in the house before the operation, he got out of and into bed by himself, walked about the ward, and on the day of the operation walked by himself into the room from his ward. The swelling might be either a small aneurism or a tumour immediately behind the artery. There was a very distinct pulsation to be both felt and seen. There was a decided lateral pulsation, and there was an evident distensile pulsation. It was not a mere elevation of the swelling, but an enlargement from within. With a stethoscope on the part a distinct bruit was perceived—very different from that in the right thigh. Pressure on the femoral below the swelling increased the size of it, and also the distinctness of the pulsation. Pressure on the iliac artery at once lessened the swelling and the pulsation, both of which were absent so long as this was kept up, and returned on its being removed. In the popliteal space the artery was less perceptible than that of the right side. The limb was colder and weaker than the right leg; it was not swollen. These symptoms, with the sudden accession of the complaint, induced me to consider it to be a small aneurism, probably springing from an opening on the posterior side of the femoral artery, very high up, and not containing much, if any, coagulum. Mr. Samuel Hey then saw the man with me; and, in order not to influence his judgment, I left him to investigate the case, not telling him the opinion I had formed of it. Without any hesitation he arrived at the same conclusion as I had done. Subsequently my other two colleagues did the same. You were present at the consultation, and heard the remarks made. It was therefore determined to tie the external iliac artery. This I did on the 22nd of November. The operation was easily done. The peritoneum was reached and pushed aside, when the finger came upon the artery. It was felt to be of more than ordinary size. Neither it nor the vein was seen. I carried the ligature from the outer side, and brought the aneurism-needle up between the artery and the vein. This is a method which I commonly adopt, though you will see in books it is recommended that the needle should be, in such operations, passed between the vein and the artery, so as to bring its point up where the vein does not lie, that this may not be wounded by it. Now, as I think there is more danger of the flaccid thin vein being caught by forcing the point of the needle in between the vein and the artery than there is by bringing the needle up between them, when the needle is kept well close to the artery by passing it from the outer side, I prefer to so carry the thread. By so doing, if the needle-point be blunt, I do not know how the vein can be wounded. If there is to be a large separation made between the artery and vein by dissecting them from each other and from their sheath, you may easily pass the ligature in the way commonly directed; but, as I think it of primary importance not to separate them from each other, nor either of them from the containing sheath, I believe you may more easily if you do not separate them, or even see them (particularly the vein), safely pass the needle as I recommend you to do, for the firm arterial coat will safely guide you. By so doing, you will, I think, have very few cases of the so-called secondary hemorrhage from ulceration or sloughing of the vessel, which, in my judgment, is more frequently caused by the connexions of the vessel having been destroyed during the operation than by all other causes put together (except, perhaps, the near existence of a branch). Of course, if a surgeon thinks it necessary actually to see an inch in extent of a bare artery before he feels sure of his position, and cannot see with his finger-ends (as it was long ago not inaptly called), but must have a full revelation to the eye itself, then to proceed to place a ligature round an artery in the manner I recommend would not be prudent in him; though to another, whose fingers are *habile* to the work, it is easy enough, and far better for the patient.

Flexible iron sutures and plaster closed the wound. On tying the ligature the tumour instantly lessened and the pulsation ceased, so that everyone present was confirmed in the correctness of the diagnosis. A woollen stocking was kept on the limb, and it was enclosed in two flannel bandages with a layer of cotton wool between them. It was a little flexed upon a pillow. At first the temperature of the limb fell eight degrees (Fahr.) below that of the other, then rose two degrees higher. As the heat increased, the wrapping was gradually removed until the temperature of the two limbs was equal.

The patient expressed himself as much relieved. The chloroform excited great nausea and retching, for which a subcutaneous injection of one-third of a grain of morphia was used, with considerable benefit. He had a dry mouth, with great thirst; no appetite; and a very quick pulse. There was no attempt at effusion of plastic lymph about the wound; but as there was an escape of thin dirty matter from it, it was dressed on the fourth day. On the sixth day, as this discharge continued, the plaster and sutures were removed, and a poultice was applied. There was no attempt at union. Though the frequency of the pulse abated, the sickness disappeared, he could take some food, and was not so thirsty; though there was no peritonitis, and the temperature of the two limbs was equal,—still I was not satisfied with his condition. There was no delirium or confusion of mind, and he had some sleep; yet there was an anxious countenance, a hesitating way of answering questions, and the toes became dark. He showed no power. On the ninth day he died, it being not very obvious why he did so, unless from exhaustion.—*Lancet, Sept.* 28, 1867, *p.* 390.

62.—ON THE TREATMENT OF ANEURISM BY ELECTROLYSIS.

By Dr. John Duncan, M.A.

[The patient, the account of whose case commences the following article, was 52 years of age, and stated that he had discovered pulsation in the upper part of his chest a month before his admission into the Barnhill Hospital, Glasgow. When first seen the tumour projected about two inches from the surface of the chest, was nearly round, and had a diameter of more than four inches. It was situated to the right of the sternum on the level of the second and third ribs. The pulsation in it was strong throughout, and the aneurismal bruit loud and distinct.]

On account of the rapid increase of the tumour, the apparently small amount of coagulum in it, and the comparatively slight disturbance of neighbouring organs, I agreed with Dr. Macgregor that the tendency to death was by external hemorrhage, and we determined to attempt its delay by means of electricity.

The operation was therefore performed on the 21st of June 1866. The patient was placed on his back, and two needles were introduced from the outer side, parallel to each other, and about one inch apart. The positive needle, which was coated with gutta-percha, traversed the centre of the tumour; the negative, coated with glass, was placed a little lower. The electric circle was closed at 11.58 a.m. by means of a key interposed in the wires.

Six minutes after its closure he began to complain of pain, the skin seemed to grow more tense, and a slight tympanitic note could be elicited by gentle percussion on the tumour. All these signs of galvanic action became more marked as we continued the application. He at first described the pain as a "burrowing," a "boring," a "weight on the chest;" but three minutes before the end of the operation he said it had become acute and burning. The electric current was stopped at 12.15, seventeen minutes after its commencment, a slight lividity began to show itself round the negative needle, the glass coating of which had cracked in introduction, and slightly separated from subsequent movements. On withdrawing the needles, a portion of the gutta-percha which covered the positive was left in the wound. It had become softened during the operation. Then also a quantity of gas escaped into the cellular tissue, and crepitated on pressure.

The patient passed a restless night—partly on account of another sufferer in the ward. Next morning his pulse was 100, and he had little appetite for breakfast. From the escaped gas and consequent œdema, the tumour measured nine inches from right to left, but was not much, if at all, changed from above downwards. The skin was tense and tender to the touch, so that he could not bear even slight pressure without a sickening sensation. The pulsation was not very notably diminished. The same evening the swelling began to decrease, and the pulse to fall; and by the 24th, the tumour was much as before the operation, retaining only an oval shape. The patient stated that he felt relieved, and especially that the shooting pains in arm and head were very greatly better. He complained, however, of uneasiness at the sternal edge of the tumour.

On the 27th, a small abscess formed at the point where the positive needle had penetrated, and an insignificant dry slough at the other puncture began to separate. Under a light poultice the slough came away and the abscess discharged itself; and, by the 29th, the aneurism had completely resumed its original shape and size.

From the 7th to the 24th of July it slowly increased again, becoming more uneven on the surface, while the pains returned to their former severity. The increase was mainly upwards, and the lower part felt moderately hard, and had a less distinct pulsation than the upper. On the 24th, I extracted, by a minute incision, a little ball of gutta-percha, which had remained under the skin since the operation, and had become encysted. I also repeated the galvanism, using needles coated with vulcanite, and inserting them in the upper or enlarging portion of the tumour. A battery of six Bunsen's cells was used (four had been the former number), and the action was continued for twenty minutes. It was stopped because the patient began to complain of a feeling of faintness, though the pulse did not diminish in force. The train of occurrences was nearly as has been already described,—rapid swelling with rapid subsidence of the same, pain on pressure and tension, relief, but not so marked as before, from the shooting pains, and for a short time a eming arrest of growth. Dr. Macgregor notes on the 25th, e day after the operation,—"Tumour of an oval shape; pulsation very slight, and more like the general lifting of the whole bulk of a solid body than the expansive feel of an aneurism."

Soon after this, however, the nodulated character of the tumour became more marked, and three eminences, each about one inch in diameter, were especially prominent by the 10th of August, when also they began to assume a dusky red hue. One of these was on the right side of the tumour, another near the centre, and a third, the smallest but most elevated, lay close to the sternum. The pains now became more severe, and were accompanied by numbness in the course and distribution of the ulnar nerve; and, in short, all his symptoms were rapidly getting worse.

On the 11th, while getting up, he felt something give way in the tumour, fell back in his bed, and for some time had the appearance of one dying. He was with some difficulty restored by stimulants.

On the 12th, a portion of the central nodule assumed the appearance of a blister filled with dark serum.

On the 15th, the prominence to the right began also to point, and next day a small opening formed there, from which a drop or two of dark fluid escaped. This was followed, on the 17th. by a more copious though easily arrested hemorrhage. For some days there was no recurrence of the bleeding, but new nodules and blisters formed close to the central one, and, by the 20th, a large superficial ulcer covered the centre and left of the tumour.

At this time Dr. Macgregor wrote to me, and I suggested that the operation should be repeated. He did not, however, feel himself warranted in doing so alone, and I was unable to go to Glasgow.

The rest of the case is a story of repeated hemorrhages. On the 22d, there was bleeding from the little orifice to the right, which is described as being occupied by a coagulum that advanced and retired with each pulse in the aneurism. The 24th witnessed yet another from the same source, and, on the 25th, a stream of blood spouted from it to a distance of fully two yards. Two hemorrhages from the central ulcer took place on the 28th, and the last was so copious that, after soaking through the bed, it formed a pool on the floor. From this the patient died.

The post-mortem examination was made by Dr. Macgregor six hours after death. The large central ulcer measured three inches across, and in it were three orifices. One was patent, the two others were plugged by coagula. The orifice in the right of the aneurism admitted a large goose-quill and was also occupied by a clot. The tumour was sliced like a brain. In t centre was a firm mass having the appearance and consisten of liver, but with a circularly striated arrangement. This was surrounded by liquid blood, which filled the space between it and the wall of the tumour. The sac itself could contain a closed fist, and the central mass was about the size of a hen's egg. The aorta was largely dilated and lined by a perfect sheet of calcareous matter, which shone like porcelain and rang to the stroke of the knife. It communicated with the external sac by a smooth opening the size of a two-shilling piece situated close to the origin of the innominate artery. The second, third, and fourth ribs were eaten through, and the right edge of the sternum and lower border of the clavicle were in a state of carionecrosis. The heart was enlarged and the semi-lunar valves were calcareous.

In sending me the notes from which I have compiled the history of the case Dr. Macgregor says,—"The post-mortem examination clearly proved that a considerable quantity of blood had undergone coagulation by the galvanism; and had the orifice in the artery been not quite so large, and the galvanism been more frequently or longer applied, success might have the result."

It is evident that the patient's death was but little if at all delayed by the operation. The history seems to me to indicate the following as the course of events:—A coagulum, adherent to the wall, was formed in the lower part of the tumour by the first operation, and the aneurism thereafter extended upwards. This clot was increased in size by the second, and the pulsation was thereby notably diminished. Still, not being entirely occluded, the aneurism continued to grow in various directions, and ultimately, in consequence of this growth and on the occasion of a slight exertion, the clot became separated from the sac. The "giving-way" sensation very probably marked the occurrence of this separation, which was followed by a speedy thinning of the aneurismal coverings. I have little doubt that even then, had I been able to operate a third time, death by hemorrhage would have been obviated; but from personal causes I could not leave town so often as I could have wished.

Although there is nothing particularly novel in the circumstances of this case, it corroborates the conclusions to be drawn from the one I have recorded in my former paper. It is evident that the action of galvanism when carefully controlled is not productive of danger to the patient. The care taken to prevent cauterisation of the sac, by avoiding contact of the exposed portions of the needles with it, obviated in great measure that tendency to inflammation which had proved a source of danger in other hands. Moreover, the insulation, pretty well attained by the glass and gutta-percha,—perfectly so by the vulcanite coating of the needles,—prevented that sloughing at the points of entrance which has on more than one occasion given rise to unpleasant results by hemorrhage and otherwise.

While, then, little danger to the patient is to be apprehended from the operation, this case seems to show the possibility of occluding the external portion of an aortic aneurism. Although it cannot be absolutely demonstrated that the central clot was produced by galvanic action, its nature and position, taken in connexion with the experiments made on blood external to the body, with other like cases, and especially with the one I shall presently relate, make it extremely probable that it was so produced. How much was due to direct galvanic action, how much to subsequent deposition, it is impossible to say, though doubtless the greater part was immediately coagulated. But being there, it follows as a matter of course that, if a clot of the size of a hen's egg could be produced by two operations of twenty minutes each, more frequent and longer continued operations will have a yet greater effect, even to the filling by coagulum of any size of sac. In this respect I speak only of the sac external to the thoracic wall, for the hope of curing an aortic aneurism by galganism cannot be seriously entertained. Not to speak of the impossibility of placing electrodes in accurate position in the thorax, or of the probability that whatever might be formed would be at once carried away by the rush of blood, it is certain that any clot formed under such circumstances would become free on withdrawal of the needles and would pass on to plug up the aorta or one of its branches. Galvanism in aortic aneurism is to be regarded in the same light as tracheotomy in that disease, as a means of prolonging life in exceptional circumstances, to be used only when the obvious tendency to death is by the way which it is calculated to prevent.

But while we must in this and other directions set limits to the capabilities of electrolysis, we have also examples of its perfect applicability to certain forms of aneurismal disease; and I have great pleasure in relating a case of cirsoid aneurism in which it afforded most complete relief.

About a year ago, James Gordon, a labourer, 21 years of age, consulted Dr. Hardy, of Leith, for an eczema of the scalp. In examining the head, Dr. Hardy observed a cirsoid aneurism, and recommended the man to go to the Royal Infirmary. He was there cured of the eczema; but it was thought injudicious to interfere with the tumour, and on leaving the hospital he put himself under my care.

The tumour was situated on the left side of the head, and reached from the vertex to the zygomatic arch. It resembled very closely in shape and size the half of a large Jersey pear.

Its broad end lay upwards, and the enlarged temporal artery entered it below. The external carotid also was distended, and even the common carotid seemed large, and beat with unusual force. In front, the left anterior temporal artery curved round to enter the tumour; behind, the left occipital could be traced from it to the muscles of the neck; while the right temporal also contributed by means of its middle branch. The whole tumour pulsated strongly, and contained no clot. By firm pressure it could be so emptied of blood that the bone felt hard and firm, but it rose again at once on the hand being removed. A loud and prolonged bruit accompanied each pulse; and the constant sounding in the ear was the great cause of the patient's distress. By compression of the carotid or left temporal artery the pulsation was greatly diminished, and the tumour rendered very flaccid; but pressure even on all its distinguishable affluents did not prevent a slight pulsatory movement.

It was evidently a case in which other means would be so dangerous and uncertain that galvanism deserved a trial.

On the 20th of July, assisted by Drs. Fraser, Hardy, and Joseph Bell, I operated for the first time. The needles, having one inch of exposed surface, were entered at the vertex and placed parallel to each other, about an inch apart. Four cells were used, and the action was continued for twenty minutes. Pressure was maintained up the affluents throughout the operation. The closure of the circle produced a smart shock, causing the patient to quiver; and another, not so severe, accompanied the opening. During the action a painful burning sensation was experienced; and in this, as in subsequent operations, Gordon stated that he distinctly heard the "blood boiling" in the tumour. When the current had been continued for about ten minutes, the aneurism began to swell; and soon afterwards the tympanitic note on percussion became audible. A slight flow of blood followed the withdrawal of the needles, but it was at once checked by lint and a bandage.

The same evening there was slight œdema all over the swelling. On its subsidence next day, there was felt in the centre of the tumour a hard nodule, divided into two parts by a tortuous pulsating passage.

At a second operation, on the 4th of August, the needles were introduced one on each side of the central nodule, and six cells were used. The shocks on closing and opening caused the patient to start violently, and tremble for about a minute; but the burning pain was not greater than with four cells, and was easily endurable. The action was stopped after twenty minutes, on account of an accident to one of the cells. The effects were precisely as already detailed. The hard core was increased in size, and the rest of the tumour continued to pulsate as before.

From the negative needle not having been deeply enough buried, a piece of skin not larger than a pea was cauterised and separated in a few days as a thin scale.

On the 8th of August one needle was placed in the middle pulsating passage, the other towards the back of the tumour, and farther additions were made to the consolidated mass. On this occasion the patient became a little sick from the pain, and the circle was opened after eighteen minutes' action, to relieve his uneasiness.

I was now for some time prevented from seeing the patient, but by the 28th of August no change had occurred; and on that day, with the help of Dr. Andrew Inglis alone, I put the patient under chloroform, to avoid the shocks, which he greatly dreaded, and proceeded to repeat the operation. The initial shock, however, caused him to throw up his hand and draw out one of the needles; and as we found ourselves unable to control him, and at the same time manage the battery and the chloroform, I contented myself with binding up his head to stop the bleeding.

On the 31st, another failure occurred; for the patient had, contrary to orders, eaten largely before the operation; and after about five minutes' action it was found impossible to keep the needles in position, on account of his motions in vomiting.

On the 5th of September, however, a most successful repetition was made. The positive needle was introduced about an inch above the zygoma, and placed with its point directed upwards; the negative had its point directed downwards in the front of the main tumour. They overlapped about half an inch, and were more than that distance apart. A current from four cells was passed through them for twenty-five minutes. The immediate effects on the aneurism were much as before; but when I saw the patient in the evening, he stated that the bruit had entirely disappeared; and on applying the stethoscope I found that it was so. Two days afterwards, when the œdema had quite subsided, I made a careful examination. The neck of the tumour above the entrance point of the positive pole was consolidated; below that point it had collapsed. The temporal and carotid arteries had resumed their normal size. The centre of the tumour was entirely occupied by a hard mass, but between this and the surface was a fluid pulsating layer supplied by four tortuous and enlarged vessels, viz., the right temporal, the occipital, and two branches of the left temporal, which ran one before and the other behind the obliterated neck, and entered at opposite points half-way up the tumour.

On the 12th of September, I proceeded to obliterate this superficial layer, by introducing the negative needle pretty deeply behind, passing it even through the coagulum, while the positive was placed in the fluid portion slightly anterior to the middle line of the aneurism. After twenty minutes' action I observed a slight lividity of the skin over the positive needle, and at once stopped the current. Unfortunately it was too late, for next day it became evident that the skin had been cauterized over a space about the size of a shilling. The whole mass was now, however, consolidated, except where the anterior and right temporal arteries entered, and neither portion was larger than a threepenny-piece. I obliterated the anterior a week afterwards, by passing under it a needle and compressing it for twenty-four hours by means of a piece of cork, secured by a figure-of-eight silk thread. The same process was not successful with the right temporal, and the patient would not allow me to repeat the attempt, as he had returned to his work, which he did not wish to be interrupted. The rest of the history is entirely favourable. The slough, much smaller than had been anticipated, was very dry and tough, and took a month to separate. But, six weeks after the last galvanic operation, the cicatrix was the only trace that remained of the tumour, with the exception of a slightly enlarged right temporal and a barely distinguishable occipital artery. The man has continued to work since that time, and there is no tendency to a development of the disease from these sources. If there were, a needle passed under or through these small vessels would serve effectually to obliterate them.

The very satisfactory termination of this case should, I think, be sufficient to establish electrolysis as a valuable therapeutic agent, well suited to the treatment of cirsoid aneurism. The only real difficulty which attended the operation was to get a maximum of effect without endangering the skin. I had previously met with the same difficulty in applying electrolysis to a nævus in the eyelid of a child. Very small needles were then used, and the current was continued for a few minutes only. Only a small central induration was, in fact, produced; and, as some impatience was expressed, I resorted to subcutaneous ligature instead of trying a more efficient application of the battery. Till the last operation on Gordon's head, I succeeded in erring on the safe side; but, notwithstanding, nay even in consequence of the mistakes in that one instance, some facts may be elicited for guidance in future. The first operation affords, perhaps, the best data for estimating the coagulating power of electricity under given conditions. As I have already said, four cells of a Bunsen's battery was used; the period of action was twenty minutes; the active portion of each needle was one inch in length, and their thickness No. 16 of the wire guage. A hard coagulum was formed, of which the breadth was an inch and a third; but a narrow uncoagulated passage remained rather nearer the negative than the positive electrode. Now this passage was, in all probability, due to a vessel lying just within the surface of the coagulum, which would thus be perfectly continuous behind it, and may in any case be very fairly estimated at one and a quarter inch in breadth and rather less in thickness. The length was one-fourth greater than that of the exposed steel, to which, of course, it will always correspond.

The intermediate operations afford no reliable data for such an estimate, on account of the relation which the needles bore to each other and to the already-formed clot; but the last is again of some importance. All other conditions being the same, the negative needle was placed perhaps not quite three quarters of an inch from the surface, the positive certainly not half an inch. The former brought the coagulum, in which it lay, up to the surface; the latter cauterized the skin.

From these facts it may be safely concluded—

1st, That both needles are efficient.

2d, That, under the circumstances named, they are together capable of producing more than one cubic inch of coagulum, the greater part of which is due to the positive pole.

The great object, and, I may add, difficulty in an ordinary aneurism, is to prevent that amount of irritation which is liable to excite a dangerous inflammation. On this ground as on every other, there seems no cause of apprehension in a cirsoid aneurism; and for that form of disease galvano-puncture may, I think, be regarded as the safest, simplest, and most certain method of treatment. Ligature of large arteries, like the carotid, is extremely dangerous, and has been singularly unsuccessful. Out of eighteen cases, seven died, and only two were permanently cured. The perchloride of iron, though it has been successfully injected, is also liable to failure, and has, moreover, certain well-known risks attending it. Extirpation, ligature, and incision have been tried, but hold out little inducement to repeat them. Galvanism, however, has been successful in the only two cases in which it has been used; and though several operations were required in each, not the slightest accident happened in either.—*Edinburgh Medical Journal, Aug.* 1867, *p.* 101.

63.—CIRSOID ANEURISM CURED BY INJECTIONS.

Prof. Pitha showed, at the Medical Society of Vienna, a young man whom he had cured of a cirsoid aneurism of the side of the head. It was of large size. The occipital, posterior auricular, and temporal arteries were much dilated, being as large as quill-pens, the temporals and occipitals showed sacs as large as nuts. The auricular was enormous, violet in colour, and showed distinct pulsations. Extirpation was not to be thought of, and ligature of the external carotid would have been useless, as the collateral branches would have carried on the circulation. Pressure, which was borne with much patience, caused œdema without diminishing the size of the tumour. Injection of the sesquichloride of iron was then tried. It formed a clot; the part became hard, suppurated at points, and the tumour disappeared. After ten injections only a little capillary dilatation remained; the occipital, which resisted longest, still pulsated.—*Edinburgh Medical Journal, Aug.* 1867, *p.* 191.

64.—ON ACUPRESSURE.*

By Dr. William Pirrie, Professor of Surgery in the University of Aberdeen.

[The following paper was read by Dr. Pirrie, at the annual meeting of the British Medical Association, at Dublin.]

The First Method may be quickly and easily performed by placing the left forefinger or thumb against the mouth of the bleeding artery, sending the needle from the cutaneous surface direct through the whole thickness of the flap, and causing it to emerge a little to the right side of the tube of the vessel. The projecting end is then pressed firmly against the site of the artery, made to re-enter the flap close to the left side of the vessel, and pressed on till it emerges on the surface of the skin. In this method, the artery is compressed against the component parts of the flap, to use the words of Sir James Y. Simpson, "just in the same way as, in fastening a flower in the lapelle of our coat, we cross over and compress the stalk of it with the pin which fixes it, and with this view push the pin twice through the lapelle." In some cases, the vessel can be conveniently pressed against a neighbouring bone instead of against the soft parts of the flap. The portion of the needle within the wound is very small; but, in many cases, it may be passed higher up, so as to compress the artery without emerging on the wound-surface. In many of my most gratifying cases, I have adopted this modification; and by so doing the wounds have not had, even for the shortest time, any foreign body within them.

The Second Method is performed with a needle threaded with twisted iron-wire, and—unlike as in the first method—the skin is not interfered with, and the needle is passed above, instead of below, the artery. The needle is pushed twice into the soft tissues of the wound. The first point of entrance is at a little distance from the artery to be acupressed, and the first point of exit close to it. The second point of entrance is close to the vessel on its opposite side; and the second point of exit at a little distance. Between the first point of exit and the second point of entrance, the needle is made to bridge over the track of the artery; and care must be taken, before making the needle re-enter the wound, to press it down sufficiently to close the artery. For this purpose, the soft parts may be pressed with the tip of the left forefinger, to aid the pressure of the needle in closing the artery before the needle is fixed by being pushed a second time into the soft parts. I trust this description will make the performance of this method perfectly clear. The needle can be removed at pleasure by pulling the twisted wire. In minor operations, and in acupressing vessels of moderate size, I have often used this method; but I have never adopted it in acupressing an important artery in a major operation; because, although I have been greatly surprised how little pressure, when direct, is sufficient to stop the circulation through an artery, there are other methods whose reliability is unquestionable, and I have, therefore, thought it judicious to give them the preference. Occasionally I have used a pin instead of a threaded needle; and I invariably do so when the form of the wound and the situation of the artery admit of the head of the pin being conveniently kept without the wound.

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* Although we have described the different methods of acupressure in a former volume, from the writings of Sir J. Y. Simpson himself, the subject is so important that we will again describe them in the words of Dr. Pirrie.

The Third Method requires for its performance a threaded needle and a loop of inelastic iron wire; and consists in effecting compression between the needle *below* and the loop *above* the vessel. The needle is entered a few lines to one side of the vessel, and, as I now demonstrate with this needle and flap, pushed behind it; caused to emerge a few lines beyond the vessel; the loop of wire is thrown over the point of the needle; brought over the track of the artery and behind the stem of the eye-end of the needle; drawn sufficiently to shut the vessel; and fixed by half a twist around the needle. It is important, in the performance of this method, to avoid including an unnecessary amount of tissue; not to draw the wire tighter than is absolutely necessary to close the artery, lest its coats should be lacerated or the tissues molecularly injured; to arrange the wires so as to prevent wriggling or entanglement, and to relieve the vessels from acupressure at the earliest moment deemed advisable, which is done without the slightest difficulty. By pulling the twisted wire, the needle is removed; and the loop being liberated is easily withdrawn.

The Fourth Method differs from the third, inasmuch as a long pin is substituted for the threaded needle. Little as the difference is, the pin should be preferred in all cases where the form of the wound and the position of the artery admit of the head of the pin being conveniently and without straining of tissues kept without the wound. Kinking of the wire at the head of the needle has sometimes caused pain in withdrawing the needle. In the method with the pin this kinking is avoided; and, besides, there are the advantages—of greater facility of insertion; being more easily withdrawn; admitting of being twirled, which greatly facilitates removal; and avoiding all wriggling and entanglement of different kinds of wires. The pin, however, cannot always be used; as certain forms of wounds and some positions of bleeding vessels render it impossible to insert the pin in the proper position and keep its head out of the wound without straining of tissues, in circumstances where the threaded needle—owing to less length of unbending material—can be used with facility. The third and fourth methods are as efficient means of arresting surgical hemorrhage as could possibly be desired; and the principle of both is precisely the same.

The Fifth Method, or Aberdeen Method, or that by the Twist, is a favourite practice with my colleagues and myself, and may be performed with a long pin or a threaded needle; but the former, when admissible, is greatly to be preferred. For making clear the description of this admirable method—which may be said to consist of three different steps—I hope I may be allowed to repeat what I have already published in explanation of this proceeding. "In the first step, the pin is inserted on one side of the bleeding artery, then pressed onwards a few lines in the same direction as the length of the vessel, and its point caused to emerge on the surface of the wound. In the second step, a quarter rotation is given to the instrument so as to place it above the artery, and well pressed down against the small portion of tissues between the instrument and the vessel. In the third step, the pin is secured and the twist retained, by sending the point into the tissues beyond the artery. The operator has, on the cessation of the bleeding, a reliable proof that a sufficient degree of rotation has been given to the needle. The first time I tried the method by the twist, a half-rotation was given to the needle; but as so little pressure, when direct and steady, is sufficient to arrest hemorrhage, I have invariably preferred, in other cases, a quarter rotation. Of all methods of acupressure of which I have experience, this is the simplest, the easiest, the quickest, and, so far as experience in the Aberdeen Hospital warrants an opinion, perfectly efficient. Simplicity, efficiency, quickness, and ease of performance, are unquestionably great recommendations of the method by the twist, when performed with a quarter rotation and a long pin;

but this variety possesses two other advantages, in my opinion, of the greatest importance, for obtaining either immediate union or union by primary adhesion. The one, that there can be but little molecular injury or straining of tissues; the other, that, by the gentlest twirl and traction, the pin can be easily withdrawn, with extremely little, if any, discomfort to the patient. Surgeons who know how little produces pain in an amputation wound; how slight a degree of pain is apt to cause involuntary contraction of muscles in the stump; how certain such contractions are to separate parts of the internal surfaces of the wound from each other, although, by retentive means, its edges are kept in apposition; and who, after the greatest anxiety to obtain the best results, have so often in a few days witnessed the disheartening effects of such contractions, will fully appreciate the value of a means of arresting hemorrhage that, at the moment deemed judicious can be removed, not only without pain, but almost without discomfort, to the patient. In this method, the artery is, to a certain extent, twisted as well as steadily compressed; and in that way, no doubt, the occlusion of the vessel and the suppression of the hemorrhage is produced."

The Sixth Method, or that by the Ring, was suggested by my able colleague, Dr. Keith—who has done much to promote the adoption of acupressure—and requires for its performance a long pin and a loop of passive iron wire. The pin is inserted a few lines on one side of the bleeding mouth of the artery, made to pass close to but not to transfix it, and to emerge about two lines, or so, beyond the mouth of the vessel. Another important precaution during this step is, to have the point of exit as near as possible to the point of entrance, so that too much tissue may not be included in the ring in the next step of the operation. The surgeon then takes an end of the loop of wire in each hand; throws the duplicature over the point of the pin; brings back an end on each side of the vessel; crosses them behind the body of the pin; draws them in opposite directions to the extent necessary to arrest the hemorrhage, *but not more on any account*; brings an end up on each side; and fixes the wire by a semi-twist in immediate contact with the front of the pin. In making the semi-twist, it is important, for a reason which will afterwards be stated, to cross the end in the surgeon's left hand in front of that in his right. In every form of acupressure where wire is used, it is important not to draw it with more tightness than is absolutely indispensable to arrest the hemorrhage; and it is surprising how little pressure is necessary when continued and direct; but in this method, more than in any other, this precaution should be observed, owing to the wire being applied in a manner more likely, with undue force, to lacerate vessels and to cause molecular injury. But, besides, if the wire should be drawn with injudicious tightness, undue constriction of the tissues it embraces might cause difficulty in withdrawing the wire after the removal of the pin. Should such difficulty occur, through forgetfulness of the necessary precaution, twisting the wire which was held in the left hand from right to left will undo the twist, and facilitate removal. This method has been found extremely serviceable in perpendicular wounds where the artery has been cut short, and also in wounds containing a bleeding vessel not sufficiently accessible to be acupressed by the third or fourth methods.

The Seventh Method requires for its performance a long pin, which is inserted twice on the surface of the body, precisely in the same way as the needle is inserted on the surface of the wound in the second method. It consists in passing a long pin through the cutaneous surface pretty deep into the soft parts, at a little distance from the vessel, pushing it on, causing its point to rise up as near to the artery as possible, bridging over and compressing it, dipping the point of the needle into the cutaneous surface on the other side of the vessel, forcing it on, and causing the pin to emerge a second time on the skin. The artery is compressed between the bone and the middle portion of the pin in front of the integument. I have published an interesting case, in which I acupressed the humeral artery by this method, with most gratifying results, on account of a wound in the upper part of the forearm attended with great hemorrhage.

[Professor Pirrie illustrated the various methods with large diagrams, and demonstrated with a needle on a flap.]

Synopsis of my Experience of Acupressure.—My experience of acupressure in important cases, of which records have been kept, comprehends its use in eleven cases of amputation of thigh; four of amputation of leg; two of amputation of arm at the upper part of the surgical neck of the humerus; one of amputation at ankle-joint; one of Chopart's amputation; two of amputation of the whole of great toe; twelve of excision of mamma; six of excision of elbow-joint; one of excision of knee-joint; one of excision of an erectile tumour; one of excision of tumour on chest; one of excision of tumour on thigh—wound eight inches long; one of excision of head of fibula; three of excision of testicle; one of hemorrhage from sloughing of hand; one of wound of hand; one of wound of upper part of forearm, with great hemorrhage; one of wound of the radial artery; and one of wound of hand, attended with great hemorrhage.

These fifty-one cases—in which alone I have acupressed one hundred and eighty-five vessels—are of a highly diversified character, and many of them are among the most important in the practice of surgery. They are admirably fitted to test the efficiency of acupressure, and to enable a surgeon to form a just appreciation of its merits. I have also employed this means of arresting hemorrhage in a large number of minor operations, as well as in many cases of wounds which occurred both in hospital and in private practice, but which I did not deem sufficiently important to put on record. My able colleague, Dr. Keith, acupressed the first case in the Aberdeen Hospital, and I acupressed the second. I have had occasion to assist Dr. Keith in all his hospital operations, and thus had ample opportunity of forming a correct appreciation of the value of acupressure in his cases as well as in my own. I have also assisted Dr. Fiddes in his operations; who, like Dr. Keith and myself, invariably practises acupressure. Such have been my opportunities of arriving at a just estimation of the merits of acupressure.

Merits of Acupressure.—Is acupressure a perfectly reliable means of arresting surgical hemorrhage? In the list I have just read of fifty-one important cases, in numerous minor operations, and in all accidents to which I have been called since my adoption of this new proceeding, there has not occurred one single example in which it has failed to arrest the hemorrhage, or where I found it necessary to substitute the ligature for acupressure; and such also has been the experience of my colleagues, Drs. Keith and Fiddes. What stronger evidence could be wished of the efficiency of acupressure for the arrest of surgical hemorrhage? And, as to the risk of bleeding on relieving a vessel from acupressure, in only one case did I meet with return of hemorrhage in doing so. It was a case in which I removed the leg in its upper third for medullary cancer. There was not a drop of oozing after the operation. Twenty-four hours after operation, earlier than usual, I relieved the anterior tibial artery from acupressure, and it instantly bled as energetically as if it had that moment been cut across. Acupressure pins and loops were in the ward; the house-surgeon and some of the surgical pupils of the hospital were with me at the time; and I immediately got the femoral artery compressed, opened the wound, and acupressed the artery again by the fourth method. The blood did not exceed a dessert-spoonful, and the whole proceeding did not occupy more than three or four minutes. This is the only instance in which I have seen any hemorrhage from an artery on its being freed from acupressure; and I attributed its occurrence in this case to the extreme exhaustion of the patient being unfavourable for adhesion. In only one case did Dr. Keith meet with hemorrhage on relieving a vessel from acupressure. It was after amputation of the forearm. The radial artery bled on removing the compression; but it was immediately acupressed again, and there was not afterwards a single drop of oozing. These are the only instances in Aberdeen. In upwards of eight hundred vessels, in two only has hemorrhage occurred on the discontinuance of acupressure; and I have no doubt that as many would be found in the same number of examples of deligation of arteries. Even without the valuable experience of surgeons elsewhere, the evidence furnished by the surgeons of Aberdeen appears to me to prove that acupressure is a means which can be entirely relied upon for the arrest of surgical hemorrhage, and that this question may be considered as set at rest.

Does acupressure accelerate the healing of wounds? This question may be put in other words; namely, Are perfect examples of healing by the first intention, or by primary union, equally attainable under deligation and acupressure? By a perfect example I wish to be understood one where not a single drop of pus has been seen. This appears to me the great question to be determined; because, although acupressure, like deligation, is a perfectly reliable hæmostatic; and although, in my judgment, acupressure has other advantages which make me greatly to prefer it: yet, although most desirable, they are comparatively of inferior importance; and the final general adoption or rejection of acupressure will no doubt depend, mainly at least, on the opinion arrived at by surgeons on this important question. As I have stated elsewhere, the use of the ligature is attended with an insuperable obstacle to obtaining perfect examples of union by the first intention, or by primary adhesion without the formation of some pus. The well-known effects of the ligature, the laceration of two of the coats of the artery, the strangulation of the external coat, the molecular destruction of that coat, and the presence of the ligature acting as a seton in the wound, render more or less suppuration at the points and tracks of the ligatures inevitable. Healing by the first intention cannot, therefore, be perfect throughout in any case where the ligature has been used. Accordingly, I never saw in my own experience, or in that of any other surgeon, where the ligature was used, a perfect example of healing by the first intention, or by primary union, without any suppuration. I believe such a case never was, and never will be, seen. On the other hand, acupressure, if properly performed, and not too long continued, does not, so far as my observation has enabled me to judge, cause laceration, strangulation, nor destruction and suppuration at the site of acupressure. No doubt, the precise local conditions resulting from the use of the needle are not, as yet, so well known as those succeeding the ligature. Opportunities of making such examinations in Aberdeen have fortunately been few; but in those which have occurred the coats of the vessels were perfectly entire. But, whatever the difference may be in the pathology of deligation and acupressure, I am perfectly convinced, from personal observations and experience, that acupressure accelerates the healing of wounds; and that under its use, aided by metallic sutures, the avoidance of all dressings, and perfect rest of the wounded part, surgeons may have the gratification of seeing, in many instances, the largest surgical wounds after major operations heal up perfectly without a drop of pus. Out of the foregoing list of fifty-one cases, in many of which "the major operations of surgery" were performed, twenty-one, comprising three cases of amputation of the thigh, eight of excision of the mamma, two of excision of the testicle, one of excision of erectile tumour, &c., healed entirely by the first intention, and without any pus. I regret that, on account of a recent family bereavement, my colleague Dr. Keith—whose experience of acupressure has been quite equal to my own—could not come to Dublin; as his report of cases of union by first intention without any suppuration, which I witnessed in his practice, would have added greatly to the testimony I bear to the advantages of acupressure. It should be remembered that these results have been obtained in the early days of acupressure, with but a very limited amount of recorded experience, and on many points none whatever, for guidance; and that as surgeons become more experienced in the practice of this new proceeding, it is to be hoped that much more favourable results will be obtained. The practical results, however, already obtained have convinced me that acupressure is an invaluable means of accelerating the healing of wounds; that it does so by removing one of the hindrances to complete union throughout by the first intention; and that the anxiety, suffering, and risk, that in future may thereby be saved, are incalculable.

Has acupressure any other advantages over the ligature? In my opinion it possesses the following:—1. A briefer sojourn of a foreign body in the wound. 2. The foreign body is of a less irritating character, owing to its being of a metallic, and not of a textile, nature. 3. Acupressure is the easiest of performance, and the quickest of all methods of arresting hemorrhage. 4. The needle is removable at pleasure, instead of after a tedious process of ulcerative destruction and more or less of accompanying suppuration. 5. The comfort which the patient early enjoys from knowing that, as far as vessels are concerned, all further interference and suffering are at an end. 6. The diminished risk of the occurrence of pyæmia, owing to suppuration not being an inevitable consequence of this mode of arresting hemorrhage. It is a remarkable fact, that as yet there has not being a single instance of pyæmia in any case where acupressure has been employed by my hospital colleagues or by myself.

In conclusion, I have only to add that, having given acupressure a fair and unprejudiced trial, I have no hesitation in stating that I am a decided believer in its advantages; that my appreciation of its merits is such, that I regard it as the greatest improvement of modern times in the treatment of incised wounds; and that—with chloroform for preventing pain during operations—and with acupressure, aided by metallic sutures, perfect rest of the wounded part, and the avoidance of all dressings, much suffering to patients may in future be averted, and the success of an important department of practical surgery greatly increased.

Discussion on Dr. Pirrie's Paper.—Dr. Buchanan: I have used the method of acupressure very extensively in the hospital of Glasgow; and my experience—perhaps owing to my want of care at first—has not been quite so favourable as that attained by Dr. Pirrie. Until I used the method of acupressure, I never saw a case of union by the first intention in all the amputations of any size in my own practice, nor in the practice of any other surgeon. There was one case of amputation of the shoulder-joint, in which I acupressed the axillary artery and two other vessels; and that case was the first case in the history of the Glasgow Infirmary of any amputation uniting by the first intention; so much so, that many surgeons in Glasgow did not believe in the possibility of such a thing, and came to visit the case. I think the thing is still in its infancy; because I am perfectly certain that union by the first intention will yet take place in all instances. I suppose it is in consequence of my want of exactness in manipulating that I have not succeeded in all instances. If you succeed in acupressing correctly, if you succeed in making an apposition of the flaps, then you may expect union by the first intention. The last case which I have related was accompanied by amputation, in the same case, of the leg below the knee. The young man on whom the operation was performed was rather restless in his bed, and, when I subsequently came to see him in bed, I found that he had kicked out two of the pins. To my astonishment, there was not a drop of blood lost. Inflammation occurred afterwards, which did not favour complete union; but it is a curious fact, that in acupressure, sometimes the pin may be removed in half an hour without hemorrhage. I think the method is one that deserves the attention of all practical surgeons.

Dr. Rawdon Macnamara: It is well known to my colleagues how earnestly I have taken up this subject of arresting surgical hemorrhage, and my experience of it is each day more and more satisfactory. In any operation I have lately performed, I have used no other means of arresting hemorrhage. In amputation of the breast, I have used no other means; and I have been presssing the matter on some of my professional friends in this city, who have been kind enough to follow out my suggestions and to employ acupressure. The way in which I was in the habit of employing it was what was called Simpson's third method. Now, the result of my more mature experience is, that Porter's twist is more satisfactory, as it is more rapid. Latterly I have been using Porter's twist. Mr. Smyly, in the Meath Hospital, used it a few mornings ago in arresting hemorrhage. This (exhibiting a needle) is what I have been in the habit of using, instead of the long needle to which Professor Pirrie has alluded. I was perfectly astonished by the way in which, some days ago, a large vessel was at once secured, and there was not a drop of blood lost. What I usually do is, to leave open the wound on which we are operating. (Some of our brethren may not be aware that we operate here at a much earlier hour than they do in London—we operate at 10 a.m.) Having secured all the needles, we go back about six o'clock in the evening to remove the needles and bring the lips of the wound together into perfect apposition. Union of the most satisfactory character takes place. In one instance, there was not the slightest appearance of a drop of pus; and in others it was very slight indeed. Speaking for myself, I may say that, in using this method, I see nothing left to be desired in the arresting of hemorrhage. The more I am acquainted with it, the more satisfied I am concerning it. In one case, a very interesting one, in which I employed acupressure in amputation of the thigh, on removing the needle, a large dash of arterial blood came, and we were all under the impression for a short time that it had not sealed the femoral artery.

Dr. GEOGHEGAN: After how many hours?

Dr. MACNAMARA: The needle had been in for forty-eight hours. But a little pressure on the femoral artery stopped the blood. I think that there must have been some twist in the wire, and I tore across another vessel. But it is perfectly idle to imagine that, if the blood came from the femoral artery, a little manual compression would have resulted in sealing it up. Two months have now elapsed since the operation. I cannot too strongly express my opinion as to the value of acupressure in arresting hemorrhage. If I were in Edinburgh, I should hold my tongue; but I should be sorry that the distinguished suggester of acupressure should be in this city, and that I, so thoroughly acquainted with its value, should sit silent while the subject was being discussed.

Sir JAMES SIMPSON, who was received with applause, said: Perhaps I have no right to speak on a section of surgery as I am not a surgeon, but I shall feel obliged if you will allow me to say two or three words. Let me first remark that, after I had worked out some problems with regard to acupressure, I happened to show the result to a lady who was a little skilled in anatomy as well as everything else; and she said, "Now, Doctor, do you want me to believe that nobody ever thought of that simple thing before?" I said that I had searched all the olden and modern authors, and to my own wonder in none of them had I found any allusion to it. Let me add that I have searched them honestly. My friend Professor Smith stated in his address to-day, as I am told, that he has found a passage in John de Vigo describing the process. I know that passage very well; and I think that my friend, Sir Dominic Corrigan, will have to change our knowledge of the Latin language before Dr. Smith will be able to prove that it relates to acupressure. Dr. Smith has committed merely an error in reading the Latin of de Vigo. It has been translated more than once. It was translated from John de Vigo's book into English three centuries ago by Trahernon, and into French by Portal. Both of these translations make Vigo say that he put a needle below the artery in order to pass and place with it the thread which was to tie and compress the vessels: and you must remember that, in the days of Paré and before him, the common way was by putting a needle around the artery altogether in order to place the thread around the vessel. De Vigo speaks of the process as one of *ligatio* (deligation); and distinctly, I think, states the thread to be left and used for tying —not the needle. But it is, after all, not a matter of any practical moment, whether the needle was ever used or not by any old authority. Let me say one thing more; that it is not seven years yet since I first offered the suggestion of acupressure publicly; and that I think I have some little right to rejoice that, in the course of those seven short years, instead of seeing the practice expire, as so many prophesied that it would, it has already extended very widely; and I have no doubt that in the hands of such very able surgeons as Drs. Pirrie, Buchanan, Porter, Macnamara and others, it will rapidly enough extend. There is no use in its spreading too quickly; let it take good hold. I quite agree in the belief that eventually we shall probably come to have acupressure done solely or almost solely with the pin, and without the wire loop. There are various other modes of acupressure besides those described by Dr. Pirrie, in common use in Edinburgh. One, done in a moment, consists of making an over or vertical twist with the single pin instead of a circular twist. I say that acupressure will probably be yet effected perfectly and entirely by a single pin; and then, as now, the rapidity with which the process can be employed will be found infinitely greater than that with which a ligature can be applied. Acupressure can be done by one man; whereas a ligature requires two. My friend, Dr. Davidson, practising surgery extensively in Madagascar, with no educated man near him to give him assistance in his operations, told me that he there got on very well with acupressure, whilst he could not have got on with the ligature in this isolated position. I may now speak of the time for removing the pin. I have seen it removed repeatedly from a breast, after amputation, within a few hours. I think there can be nothing more curious than that case of Dr. Keith's, which has been instanced, where the boy, whose thigh was amputated, removed the pin himself from his own femoral artery after four hours. Why, that boy was already one of the greatest surgeons alive, practising thus philosophically as he did on his own thigh and femoral artery. Dr. Pirrie referred to the results which may ensue from the pin being left in too long. My friend, Dr. Humphry of Cambridge, one of the most accomplished surgeons living, in the last number of our journal spoke of its having been left for five days over a femoral artery. My wonder is that, when left so long there, it should not have had irritating results. It is against every rule that obtains in relation to the process to leave the pin inserted such a length of time. I published, at the suggestion of Mr. Compton, the rule that Dr. Pirrie speaks of, of not removing a needle while its knob throbs and moves; just as Homer describes the wounds of some of the Greeks, where the spear had been buried in the heart, that the weapon pulsated until the heart ceased to beat; so I believe that, in the case of the acupressure pin, while pulsation continues in the head of the pin, it, as a general but not absolute rule, perhaps should not be removed. With regard to the so-called methods of acupressure, termed numerically the first, the second, and the third, &c., methods, I believe we must classify them in some other way, as confusing the subject. Some time ago I saw a surgeon compress by two pins the humeral artery in a case of aneurism of the fore-arm. He inclosed, without any previous cutting, the humeral artery by a pin on each side of it, fixed the points in a cork, and then brought the heads together till all pulsation in the aneurism ceased. It may become yet a question whether all arteries in limbs could not be thus secured by needles alone, and without any previous use of the knife. In the case alluded to, the needles compressed the humeral nerve as well as the artery, and with what result? The woman before the operation was lying moaning with pain, apparently from pulsatory distension. The moment the pressure was effected, she said that the pain was gone; so that, instead of the compression of the nerve having given increased pain, it seemed to relieve the pain at once. A gentleman from India who, I was told, was to be at this meeting of the Association, has, I believe, done in various instances what I proposed to do long ago in the matter. Having no assistants with him, he has repeatedly placed the pin over the femoral artery when he was going to amputate the thigh, using it thus as a tourniquet. He always took it out afterwards. There I think he was wrong. I do not think surgeons have yet to learn to put a large needle over the femoral or other artery, and then by leaving it, the needle would become both a tourniquet and a ligature to the principal artery, and thus make surgery comparatively bloodless if it succeed. Remarks have been made as to the effects of acupressure in promoting the entire primary union of the wound. We are collected in a section of surgery, and I should like to ask any surgeon here, whether, in any hospital with which he is connected, or in his private practice, he has ever seen a single case of entire primary union taking place in any surgical wound where a ligature was used? Dr. Pirrie says he does not believe any man ever did see such a case; and I scarcely think it possible in the case where ligatures are employed. I have once or twice nearly got into a scrape on the subject with some gentlemen from North America (who often try to take their fun out of you); for, when they came to speak about it, I have said that I knew, of a perfect certainty, that when the Northern surgeons cut off the limbs of poor Southern soldiers, they always put five or six little morsels of dead flesh into the sides of every amputation wound. But what was the explanation? That all of them used ligatures, and each ligature necessarily strangles a small morsel of flesh; and with five or six bits, however small, of dead septic decomposing animal tissue in a wound, no surgeon in his senses would expect it to heal by complete primary union. Dr. Pirrie mentions that he has, since using acupressure, amputated twelve mammary tumours, some of them of large size. Eight of these twelve cases of amputation of the breast recovered by entire primary union. Now mark what Dr. Pirrie speaks of as "entire primary union." In some of his first cases of amputation of the thigh, he says with respect to one, that he had only half a tea-spoonful of pus; but the presence of pus at all prevented such cases from being looked on as being cases of *entire* primary union. The surgeons of Aberdeen are all agreed, and the surgeons of all hospitals should agree, that no case can be called a case of complete primary union where a single drop of pus appears; and, in eight out of Professor Pirrie's twelve cases of amputation of the breast, there was not a single drop of pus. The primary union was entire, and effected at once. Surely this is a great march forward in surgery. I believe that, up to the time when acupressure has been used, no man in any hospital either in Dublin, London, Edinburgh, or elsewhere, has eve observed similar results with the ligature. If Dr. Hingston from Montreal is here, he will tell you that I showed him in Edinburgh, a month ago, two or three cases of removal of mammary tumours in which acupressure was employed by me, and in which there was not a single drop of pus. If surgeons are strangely apathetic as to the desirability of attaining such attainable results, patients are not equally so. In one of these cases, in which it was deemed necessary to remove the breast, more than one leading surgeon saw the case; and a most skilful and excellent surgeon was selected to perform the operation. After it, six or seven weeks of daily dressing of the wound ensued. The patient told me that at last this daily dressing and irritation became so frightful a process to her, that she used to shake when she heard the surgeon's carriage stop at her door. In three months, the tumour grew again; and no persuasions of Dr. Wood or of mine could induce her to submit to the same process again. Eventually, however, I undertook to remove the renewed growth again without ligature or dressing; for there is no dressing when acupressure is used. Except some stitches with iron wire, no kind of dressing was used. Not a drop of pus followed; and the disease has not returned, though eighteen months have elapsed since this second operation. The very irritation of the dressings, the patient argued, speedily brought it back the first time; and I wish this section could hear her dilate upon the professional iniquity and cruelty of dressings, when none such are necessarily required. I was lately told by a medical friend of a case of a gentleman who had a tumour some time ago removed in Edinburgh, and who, after being operated on, was weeks in getting well. After returning home, he happened to get hold of Dr. Pirrie's book; and, after reading it, angrily argued with his ordinary attendant, my informant, "Why was I tortured for six weeks to please old surgical prejudices? Dr. Pirrie might have cured me in a day or two." Now, speaking of complete union, let me add that, in the hospitals both of Paris, London, Edinburgh, and Glasgow, there have been tried of late years various materials as local applications to raw surfaces, for the purpose of producing primary union. The material has varied from turpentine and alcohol to iodine and chloride of zinc. The material latterly used has been carbolic acid. One of the chief reasons for recommending it and others has been, that there may be sporules, germs, or fungi floating in the atmosphere, which, if allowed access to the wound, would cause irritation and suppuration. No one has yet, I believe, seen these mythical fungi with the eye or the microscope; but carbolic acid and other chemicals are believed to have the power of destroying them, and of preventing the decomposition of blood, &c., upon the surfaces of wounds. Some years ago, Dr. Küchenmeister (see *Sydenham Society's*

Year Book for 1860) wrote on carbolic acid as a disinfectant useful in surgical practice, and fitted for arresting putrefaction and preventing the development of such fungi. My colleague Professor Spence tells me that, three or four years ago, he washed the surfaces of some stumps with a solution of it, but soon abandoned it as useless. Within the last few months, the practice has been revived by Professor Lister of Glasgow, as quite novel; for there are more strange errors in the neglect of antecedents in modern than even in ancient surgery. I got a portion of the skin of the back of my hand scorched off by a diluted application of it, and the sore suppurated more or less for four weeks. It may or may not turn out, when more diluted, an useful application, and an aid to acupressure; but Professor Pirrie has shown that acupressure by itself, and without any dressings or medicated applications, can secure complete primary union for the surgeon. Surgery cannot gainsay such evidence as his. Why should it longer resist it?

[Dr. PIRRIE afterwards observed:]

It is only since the adoption of acupressure that union without any suppuration has been seen where means were required to arrest the hemorrhage. With regard to the kind of needles best suited for acupressure. I, in common with others, have found the common sewing-needle with a round point a powerless instrument and with difficulty inserted into the tissues. Pins, where admissible, are greatly to be preferred. The pins I have been in the habit of using are those I now show you. They are made by Messrs. Weiss, of London, and are of four sizes, as you here see. My colleague, Dr. Keith, frequently employs the pins I now hold in my fingers. They are made by Messrs. Turner, of Birmingham. Spear-pointed pins are objectionable, as they are apt to cut the tissues; those with glover's points are greatly to be preferred. In every case where union by the first intention is possible, Dr. Keith, Dr. Fiddes, and myself, practise acupressure, both in hospital and in private practice; and the appliances you now see, are those which we employ. I am sorry I cannot give reliable guidance as to the shortest possible time at which it is safe to relieve vessels from acupressure. The practice at the Aberdeen Hospital for some time has been to remove the compression from the larger arteries in forty-eight hours, and from the smaller in twenty-four hours or in much shorter time. It is unquestionably safe at these periods; but I have no doubt that future experience will show that the early friends of acupressure have been over cautious in removing the compression, and that they thereby lost some of the advantages of its use. In one case I relieved the axillary artery from acupressure in fifteen hours; the spermatic arteries in two hours and a half in another case; and in an excision of the mamma, three weeks ago, the whole of the vessels were relieved from acupressure in three hours after operation. The two first-mentioned cases have already been published. The most remarkable case I know of the early discontinuance of acupressure is that of a boy, whose thigh was amputated by Dr. Keith. Four hours after operation the boy removed three of the pins, and one of them was that by which the femoral artery was acupressed, yet no hemorrhage followed. I was much gratified by the remarks one gentleman made, expressive of his surprise at the amazingly short time in which he had seen an artery acupressed by means of the method by the twist. It is not only the simplest, the easiest of performance, but the quickest of all methods for arresting hemorrhage, and it has been proved to be perfectly efficient. I cannot forget the impression produced in the theatre of the Aberdeen hospital the first time I performed acupressure by the twist. This method was suggested to me by the late Dr. Knowles, and the first time I tried it was to acupress the femoral artery in one of my amputations of the thigh. The students had not heard of this method, and they were perfectly surprised, and could not understand by what means the bleeding from the femoral artery was so quickly arrested. In situations, where admissible, no better method need be desired. In illustration of the happy results of acupressure, aided by perfect immunity from motion and the avoidance of all dressings, I hope I may be pardoned for stating briefly the outline of one of my cases, although already published elsewhere. At the Aberdeen hospital I performed amputation of thigh in a delicate looking boy, six years of age, on account of extensive disease of the knee-joint and the condition of the leg. The femoral and other two arteries were acupressed by the Aberdeen method, by the twist, and another vessel was compressed by the fourth method. The pins were withdrawn in forty-four hours, without being followed by a drop of blood, and, the patient said, without causing any pain. After operation I thought it necessary to caution the little patient not to touch the acupressure pins, which he called the pins with the beautiful heads, and promised to give them to him after their removal. He took care not to touch them, or to allow any person except myself to do so, and, to his great delight, sent them home by his father for preservation until his return. Since the operation, the little boy has slept as well as ever he did in his life; has been anxious for his food; has had no uneasy sensation of any kind, and has been in the highest of spirits. The stump healed perfectly and throughout, either by immediate union or by primary adhesion. The healing must have been by one or other of these processes; and I am inclined to think it must be by the former, as no medium of union is discoverable at the edges of the wound. No dressings have been employed, or retentive appliances of any kind, except the few sutures and the three strips of isinglass plaster which were applied before the patient was taken from the operation table. The sutures were removed on the fourth day. This little patient has a fine musical ear; and a few days after the operation, when I entered the ward with the surgical pupils of the hospital, he was whistling most beautifully "The Braes o' Mar," and amusing himself by passing a little model carriage over an inclined plane which he had got constructed on his bed. He continued to whistle the above and other airs until it was his turn to be visited. This is one of the many instances we have lately seen in the Aberdeen hospital of a capital operation, and after treatment having been made delightful to the patient as well as to the surgeon. By the aids of chloroform, acupressure, no dressings, and the most perfect immunity attainable from every movement of the stump, many of the hindrances to these gratifying surgical scenes are removed. Since the use of dressings was discontinued in the Aberdeen hospital, patients have looked forward to the visit of their surgeon as the happiest event of the day, instead of, as formerly, dreading the visit and being thankful when it was over.—*British Medical Journal, Aug.* 31, 1867, *p.* 171.

65.—ON A NEW METHOD OF CLOSING BLEEDING VESSELS BY MOVEABLE FORCEPS.

By Thomas Nunneley, Esq., Surgeon to the Leeds General Infirmary.

[Mr. Nunneley has not been satisfied with the results of acupressure. The difficulties of effectually closing the blood-vessel are in his judgment so great as to counterbalance the undoubtedly great advantage of having the wound free from the long continued presence of a foreign substance.]

Though Drs. Pirrie and Keith assure me that in their hands no case of hemorrhage, either primary or secondary, has happened; and that all, even large amputations, have not only recovered, but have healed by the first intention, without sloughing, pyæmia, erysipelas, or other misadventure; the results have been far different in other hospitals and in other hands, where some of the stumps have not only had to be opened out to more effectually secure bleeding vessels (which, it might perhaps be asserted, happened to be so by the want of skill or care on the part of the operator), but where the withdrawal of the pins has been followed by hemorrhage so copious as to prove that no permanent closure of the vessel had been effected; while union by the first intention, which is the great desideratum, has not only not been obtained, but as much suppuration and delay in healing by granulation has taken place, as in the ordinary method; gangrene of the stump has occurred, and pyæmia and erysipelas have been set up. I must confess to my experience more nearly corresponding with the latter results, than with those of Drs. Pirrie, Fiddes, and Keith. I, therefore, infer that the great success which, I am assured, has been experienced in Aberdeen, is dependent upon some other influence than acupressure alone; indeed, I cannot but feel too much is proved by the reported enormous success, which is too great to arise solely from the employment of acupressure. However advantageous it may be, it alone could not exempt large operations from danger, and ensure uniform recovery. Some other accidental, overlooked, and unknown, but most potent influences, must have been at work. As there are seasons of great want of success, in which, from one cause or other, often so subtle as to be unknown, almost all wounds, however unimportant, do badly, so there are other times when all, however serious, do well. I cannot but suppose that at least part of the Aberdeen success must be attributed to such a condition rather than to the use of acupressure alone.

Immediately after Sir James Simpson's introduction of acupressure, I had correspondence with him on the subject; and he has favoured me with letters from time to time since them; so that my attention has been constantly directed to the subject. My first idea was that, if the fibrous material were the cause of pyæmia and other calamities, as Sir James Simpson has emphatically declared, the use of metallic ligatures would at once obviate the mischief; and as animal bodies are more tolerant perhaps of iron than of any other of the metals, I got iron wire drawn of various degrees of fineness, even up to what was presumed to be No. 45 (for no gauge is made nearly so fine), and, by careful annealing, managed to obtain it as flexible as common thread or silk, without rendering it brittle, so that it will draw into a double knot as easily as they will. With this, in amputations, I tied many blood-vessels. In two cases of amputation of the thigh, the femoral arteries were secured by the wire; I found it to cut well through the outer arterial coats, and to hold firmly; but, while it thus proved effective as a ligature, I did not perceive any material advantage in the healing of the wounds, and it was accompanied by a very important disadvantage, for the very property of being so tolerated by the system without exciting irritation, which gives the metal so great a superiority over fibrous material for sutures, is attended by a corresponding disadvantage where it is used as a ligature on vessels; for instead of separating within the usual period required for the coming away of thread-ligatures, the iron-ligatures would remain on for an indefinite period. Some of them I was obliged to cut off as close as I could and leave on the vessel, so that a complete cure was unduly delayed; and I have almost given up the plan in consequence.

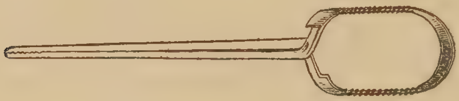
More recently it has suggested itself to me that, if I could contrive self-acting forceps sufficiently fine in the stem not to keep open the wounded parts, sufficiently strong to keep the divided vessel perfectly closed, and yet not so powerful as, by their pressure, to cause sloughing or ulceration of the included vessel, but merely adhesion of its coats and closure of its mouth, firm enough to remain securely on the part to which they are applied, and yet capable of being withdrawn at any desired period without in any way disturbing the wound, or to interrupt the desired union by the first intention, the hemostatic power wanted would be obtained.

After a trial of various forms, I think that I have succeeded in devising a form which will supply all the required essentials. I need not detail the different forms of compressing power which I have tried, nor the defects of one kind or other that I have found in them; but simply give a sketch of that one which, after sufficient trial, I have found to answer. As the vessels lie at different depths from the surface and are of different sizes, the forceps must be of different lengths and strengths. Obviously, to reach and hold the femoral artery, a larger and stronger instrument must be employed than that which is required to compress a cutaneous and subcutaneous branch, and so for vessels of intermediate size and position; but, I think, four, or at most five, sizes, will be found sufficient for all purposes. They should range from three inches to three-quarters or half-an-inch long. While the latter will be adapted for small superficial vessels, the larger will be large enough for the femoral or iliac arteries, even in stout men. The points of the forceps should just embrace the vessel, while the bowed shoulder should just project out of the wound; a greater length would be useless and inconvenient by being in the way, while it is necessary that the bow should project from the skin, or it will keep the lips of the wound apart, and not allow of the forceps being readily removed from the vessels, and so, in the attempt to do this, possibly, by disturbance of the vessel, cause hemorrhage, or arrest the healing process in the wound. The forceps are self-holding; to remove them, it is only necessary to press upon the bow. This may be made broad, rather flattened at the sides, and, if desired, somewhat roughed, so as to afford a firmer hold; while the blades should be made as fine, and lie as close together, as is compatible with the necessary strength. This, however, need not be great, for very little power is necessary to effectually close the mouths of even large arteries, provided it is only uniform and regular in application; a certain strength is more requisite for giving firmness and stability to the forceps than for making great pressure on the blood-vessels. The points of the blades should be roughened, as in the ordinary dissecting forceps, so that they may hold better. I had at first small teeth made so as to seize the vessel, but this I find to be unnecessary in ordinary cases; but perhaps, where a large number of vessels have to be secured, and much handling and disturbance of a stump is required before all is made safe, the small teeth would cause the forceps to be less liable to be displaced while this is going on. So soon as the surfaces are brought together, there can be no danger of disturbance. Hitherto I have had them made of steel by a cutler, which, of course, is somewhat costly at the first; but as they can, after well washing, be used any number of times, this result is not a consideration, and if they had a thin deposit of gold upon them all fear of rust, &c., would be removed. I am by no means sure that, for the short time the forceps remain on the vessel, steel is necessary; I think it probable that manufacturers of the safety-pin, or like handicraft workmen, may be able to supply a forceps sufficient for all purposes at so small a price, as to allow it to be used without any consideration of economy. This I am taking steps to ascertain. At one time I thought of employing forceps whose blades are brought together by a screw motion or by a sliding tube; the latter idea had also occurred to Mr. Birkett, as he told me the other day when we accidentally met on a steamboat on Loch Lomond; but, like him, I abandoned it, for I found it had disadvantages, and, moreover, would be very costly. Doubtless other forms will suggest themselves to other persons, and modifications may be made; whatever may most effectually secure the requisites I have named will be so much gained.

From small arteries I have removed the forceps in from twelve to twenty-four hours, but, in the larger, twenty-four to forty-eight hours should be allowed to elapse before they are taken away, by which time, I apprehend, the largest artery may be regarded as effectually sealed, though, if thought requisite, I see no reason why they may not be left *in situ* for a longer period. Hitherto all the cases in which the forceps have been employed have done remarkably well, but a longer time and more experience will be required before we can dogmatise upon how much is due to the employment of the forceps, and how much to other circumstances.

I think it by no means improbable that, in some cases of aneurism, compression, by similar forceps, of the artery, on some point of its course above the sac, for a greater or less length of time may be successful. The sheath of the vessels should be merely punctured to an extent sufficient to allow of the forceps being glided upon the artery alone, no other part being included or disturbed. If, after a sufficient trial of the forceps on divided vessels, I find the result as satisfactory as I anticipate, I shall certainly feel disposed to use them on an aneurismal artery.

FIG 1.



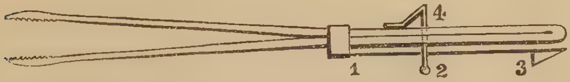
Forceps with roughened points.

I would just add another word. After the wound is properly brought together, the forceps are very much less likely to be disturbed in the necessary or accidental movements of a patient than are the needles in acupressure; that no other tissue than the vessel itself is compressed, as it necessarily is in that process; and that, if venous hemorrhage be troublesome, there is no reason why the forceps should not be applied to the open vein as well as to a bleeding artery, for the effects would not be like those to be feared from a ligature, and the forceps may be removed on the completion of the operation or very shortly afterwards.

I append a sketch of the forceps, which sufficiently explains their construction, only pointing out that the compressing power is obtained by the cross joint, as in the little bull-dog forceps of Liston; and, if more power be desired than the simple bow gives, any amount of pressure may be obtained by giving it a second or third turn, making the turns larger, but this I apprehend will not often be required. To make security doubly sure: if any one feels any doubt as to all fear of hemorrhage being removed by giving the forceps one or two turns after seizing the vessels and bringing them out of the wound, he will continue torsion with pressure.

As I have heard fears expressed lest the long legs of the forceps should give way, and allow the open vessel to escape and hemorrhage to occur—a fear, however, in which, if the forceps are properly constructed and applied, I do not participate—I have had another form of forceps made. These are shown in fig. 2. They are fine forceps, with long straight blades, on the upper part of which is a sliding clip, which is to be pressed down as soon as the vessel is seized by the points. By this arrangement, any amount of pressure may be applied, and slipping is impossible. The clip in those shown in the figure is kept steady by a slot, which slides upon a projecting part of the rivet which unites the two blades; but I think this slot may be done away with, and the clip made simply to slide upon the two blades, by which arrangement there would be less chance of the forceps becoming fixed in the wound by the coagulated blood.—*British Medical Journal, Oct.* 12, 1867, *p.* 310.

FIG. 2.



Forceps with compressing clip: 1, a moveable clip, having a slot which slides upon 2, a projection of the rivet, which unites the two blades; 3, a roughened button upon the top of the clip; and 4 a fixed one upon the opposite side of the forceps, by which these can be more easily held, and the clip pushed down towards the points of the forceps.

66.—ON LIGATURE OF THE FEMORAL ARTERY ACCORDING TO THE PLAN OF THE LATE PROFESSOR PORTER.

By Dr. RAWDON MACNAMARA, Surgeon to the Meath Hospital, and Professor of Materia Medica in the Royal College of Surgeons of Ireland.

In Mr. Erichsen's work on *Surgery*, when speaking of the propriety of deligating the common femoral artery, after stating the objections to the procedure, he winds up with this remarkable passage: "This operation, I think, therefore, should be banished from surgery." Whether this sentence is as just as it is sweeping, is the object of the present communication. It is true, in the present position of surgery with respect to aneurism occurring in the popliteal space, that ligature of the femoral artery has been deprived of a great portion of its importance in consequence of the success attending its treatment by compression; and it is equally true, that the surgeons of this city have borne no small share in bringing about this most important improvement in practice. Still, cases will occasionally occur which are either unsuited for treatment by compression, or in which, no matter how diligently or scientifically employed, it will fail in effecting a cure; and then our only resource is occlusion of the main vessel, either by the old-fashioned, time-honoured ligature, or by the more modern system, acupressure. When such cases present themselves, therefore, it is all-important that that form of operation should be selected which, beyond all other considerations, is attended with the minimum of risk to the patient's life, and which (a minor consideration, to be sure), is easiest of execution by the surgeon. Should ever the operation, the merits of which I am about to bring under notice on the present occasion, be recognised as the proper one to be selected under circumstances calling for deligation of the femoral artery, it will be a subject of interest to look back and observe how, as the science of operative surgery progressed, the tendency has been to ascend higher in selecting the site for its deligation —from Hunter's canal to Scarpa's space, and now from Scarpa's space to what, I doubt not, will yet be known as Porter's space.

The manner of securing the vessel in this situation will require but a few words to bring it in all its details before your mind's eye:—a transverse incision, about two inches in length, parallel with, but half an inch below, Poupart's ligament; the incision being so effected as to leave the artery in its centre. The method of performing this incision is simple in the extreme. A fold of skin is pinched up—the operator himself holding one side of the fold, his assistant the other; and it is transfixed with a sharp-pointed bistoury; and, by cutting outwards, the first incision will be completed. The most ordinary care will ensure the artery's being in some portion of the wound. A very few touches of the scalpel, with the assistance of a director, will now expose the vessel; and it can be secured in any way that the operator may select. The great advantage of this operation is that, in this situation, the artery is widely separated from its vein; and consequently there is no danger of wounding it. When I say "widely" separated, of course I speak in an anatomical sense; the space lying between them in reality being about two to four lines. All operating surgeons know the danger that exists of wounding the vein when either Hunter's or Scarpa's space is selected as the site of operation; and the records of surgery are not wanting in numerous cases where, in the most skilful hands, this accident has, I may almost say, unavoidably occurred. The lamentable results of such an accident as including the vein in the ligature require not to be dwelt upon here by me. All this is gotten rid of by selecting Porter's space. The vein cannot be wounded, much less included in the ligature; and thus deligation of the femoral artery is happily freed from one of its most serious risks. The facility with which the artery is reached also is a recommendation. After the first incision, a very few and very trifling touches of the scalpel bring us down upon it; in fact, the surgeon who performs this operation for the first time will be surprised at the facility with which he will in this situation reach the artery. As to the vein, he need have no apprehension about it: it is far out of his way; and, unless by the most stupid bungling, he cannot in any way injure it. As to hemorrhage, in all the cases which I have seen, the operation was all but a bloodless one; no vessel being wounded; no ligature but that for the femoral itself being called into requisition. As to the close proximity of the profunda interfering with the formation of a coagulum, theory is one thing, facts another; and as long, but no longer, as theory harmonises with facts, I will accept the theory; but when theory is unsupported by facts, I most respectfully decline to accept the theory, no matter from what quarter it may emanate; and, upon this point, I shall presently direct attention to the facts of the operation, as observed in this city. It has been objected also to this operation, that tying the artery above the profunda cuts off the supply of blood from the limb—a circumstance that would militate against a successful result; but deligation of the same artery a little higher up (for really the external iliac may be so considered) is obnoxious to the same charge, and yet this operation is one of the most successful amongst our greater surgical procedures. But, fortunately, theory in this instance harmonises with facts; for anatomy teaches us that, if the femoral artery be tied above the profunda, the current of blood would find its way through the anastomoses existing between the obturator and the internal circumflex arteries; the ilio-lumbar, with the circumflex ilii; the gluteal, sciatic, and pudic, with the internal and external circumflex, and with the perforating branch of the profunda; and, as I trust to prove to your satisfaction, in practice these anastomoses are found to be quite sufficient to carry on the circulation efficiently.

I shall now proceed to bring this operation, in its relation to surgical practice, under your consideration. It has already been performed eight times in this city—three times by the late Professor Porter, once by the late Mr. Smyly, once by Mr. Butcher, once by Mr. Porter, once by Mr. Collis, and once by myself. Mr. Butcher's case was fatal, as was also Mr. Collis's; all the rest made good recoveries, and, so far as I know, are still alive. So that out of eight cases operated upon in Porter's space we have six recoveries to two deaths—a sufficiently repectable result for an operation that should be banished from surgery! But, if we analyse the two unsuccessful cases, the results will be still more favourable. In Mr. Collis's case, as you will perceive from this morbid specimen, a most unfortunate anomaly exists in respect to the giving off of the profunda—in detaching itself from the femoral at the very point where the ligature was applied. In Mr. Butcher's case the operation was performed for a wound of the profunda artery, in which, after most persevering exertions, he was unable to reach the wounded vessel itself, and consequently he ligatured the femoral in Porter's space; and, although the patient died in eleven hours after the operation, it, so far as operative interference goes, has a just right to be looked upon as a successful case, inasmuch as the patient lost no more blood after Mr. Butcher tied the femoral, the poor man having evidently sunk from the effects of all the blood he had lost previously to Mr. Butcher's seeing him. At all events, it has no right to be considered one way or other in calculating the statistics of this mode of procedure.—*British Medical Journal, Oct.* 5, 1867, *p.* 285.

67.—ON THE PATHOLOGY AND TREATMENT OF NÆVUS.

By Thos. Smith, Esq., Assistant Surgeon to St. Bartholomew's and the Children's Hospitals.

A nævus is either a separate growth formed of bloodvessels; or an area where bloodvessels so predominate over the natural tissues of the part as to communicate to it an abnormal and permanent vascular stain.

Nævi may be composed chiefly or entirely of arteries, chiefly or entirely of veins, or of capillaries; being on this account termed respectively "arterial," "venous," or "capillary." The microscopic structure of a venous nævus—the ordinary kind that belongs to childhood—may be seen in the specimens under the microscope on the table. The growth is formed of a congeries of veins and venous capillaries running a most tortuous and irregular course in loops and tufts; the whole growth being generally surrounded by a capsule of areolar tissue. The blood to a growth of this kind is supplied by one or more large veins which may be called afferent, and is carried off by one or more efferent veins; so that the vascular system of such a nævus is just after the plan of the portal circulation, being carried on by two sets of veins with intermediate venous capillaries. This is the structure of a subcutaneous venous nævus—the only kind of child's nævus that I have had an opportunity of examining after its removal from the body.

Nævi may occur in almost every part and in almost every tissue of the body; having been found in the liver, lungs, bones, and tongue, as well as in their more common seats, the integument and subcutaneous cellular tissue. M. Guersant relates a case where, in one child, he found the disease in the lungs and liver, besides over a considerable portion of the head and face.

For practical purposes, and since nævi are generally situated either on or in the common integument, these growths are often classified as cutaneous, subcutaneous, and mixed; the last being partly in the skin and partly under it.

There is no part of the external surface that is exempt from a liability to nævus, though certain localities show a greater tendency to the disease than others. Thus, the hairy scalp and face are, perhaps, the chosen seats of the affection, next the trunk, and lastly, the extremities. They may, however, be met with in the vagina, in the rectum, or in the interior of the mouth; on the gums, the tongue, and on the conjunctiva. But in these cases they are, for the most part, in connexion with, and are prolongations from, nervous growths affecting the skin in the immediate neighbourhood.

In looking over the cases that I have noted as occurring in the out-patient room, I find a singular predominance of females over males in those suffering from nævi. Since the month of July, 1861, I have seen 140 cases of nævus, of which I have kept some record: 95 were females, 42 were males, and in 3 the sex is not stated. This difference is too considerable to be accidental, especially when the aggregate number is taken into consideration; we may, therefore, safely conclude that female infants are far more liable to the disease than males, though I confess I was quite unaware of this circumstance until I thus reckoned up some of my own cases.

In referring to the *symptoms* of nævus, one may shortly say of the *arterial*, that the only symptom distinctive of this variety is pulsation; if any nævus does not pulsate in a living child it is not arterial. These nævi are but rarely seen in adults, and are scarcely ever met with in children; it has never fallen to my lot to see one in a child.

Subcutaneous venous nævus is generally a lobular, puffy swelling, about the consistence of a fatty tumour, soft and very slowly elastic; that is, if compressed firmly, it collapses considerably, and but slowly regains its shape and consistence on pressure being relaxed. It is situated at various depths in the subcutaneous tissue; if very near the skin it generally communicates a pale bluish tinge to the surface.

Here is a little girl, about two years old, with a subcutaneous nævus; on the side of the trunk it extends from the navel to the spine in one direction, and from the ribs to the crest of the ilium and Poupart's ligament in another. It is seemingly two or three inches thick, and is as large as a good-sized plate. Until lately it was unconnected with the skin, which, as you see, is tinged of a bluish purple colour. This growth is now undergoing spontaneous cure, and the skin is dimpling down over it and forming connexions to it as the vascular tissue beneath shrivels up.

These subcutaneous nævi may be confounded with cystic, glandular, and fatty tumours, and lastly, and [illegible] fatally, with meningocele. The diagnosis between subcutaneous nævi and congenital cystic tumours, in doubtful cases, may be facilitated, and the ignominious, though sometimes necessary, resort to a grooved needle may be avoided, by observing the effect of firmly grasping the tumour, a proceeding that much diminishes the size of a nævus, while it produces no impression on a cystic tumour; and this manœuvre is often absolutely necessary in order to distinguish between these tumours, since the blueness of deep vascularity over a subcutaneous nævus almost exactly resembles the blueness of translucency seen over congenital cystic growths.

Large subcutaneous nævi, when deeply situated in the submaxillary region, bear some resemblance to glandular tumours, and mistakes of this kind have been committed. Moreau relates the case of a surgeon, an author on nævi, who made this mistake. In this case, also, the employment of pressure would be a valuable means of settling the question; while the existence of any vascular tinting of the skin over the part, or the presence of nævi elsewhere on the body, would be almost decisive in favour of the growth being nævus in structure, in a doubtful case. The same may be said of the diagnosis between nævi and fatty tumours, though the latter are comparatively rare in infants.

The following case, given by M. Guersant, illustrates the fatal resemblance the subcutaneous nævus may bear to meningocele :—

A child had on the internal angle of the right orbit a small swelling, which bore all the characters of a subcutaneous erectile tumour. Many Fellows of the Surgical Society of Paris saw this case, and agreed with M. Guersant that the tumour should be treated by thread setons. Shortly after this had been carried into effect, the child was seized with cerebral symptoms, and quickly died.

On post-mortem examination, the growth was found to be an encephalocele; the sac of which, the size of a pea, passed through the fronto-ethmoidal suture, and protruded at the inner angle of the orbit. The protrusion was formed of a small portion of cerebral substance, covered by the membranes of the brain.

Such a tumour I myself have seen on the bridge of the nose in an infant. The real nature of this swelling was only decided by a puncture with a grooved needle—a proceeding which proved it to be a meningocele, that must have protruded through the fronto-ethmoidal suture. This conjecture has not, I am happy to say, been either confirmed or refuted by post-mortem examination, since the child is still alive. In dealing with nævi in connexion with the bone at the inner angle of the orbit, it is always prudent to bear such facts as these in mind. And though I confess that in some cases it is exceedingly difficult to arrive at a clear and certain diagnosis, yet I believe that a searching investigation of the surface of the skull beneath the tumour by firm pressure will generally detect some chink in the bones if the tumour be of cerebral origin. In any case of doubtful identity, the existence of nævous growths on other parts of the body will be a strong ground for suspecting the disease to be of a similar character; while a small puncture with a fine grooved needle will always settle the question without endangering the life of the patient.

Cutaneous nævi are those that involve the skin to a greater or less extent without extending into the subcutaneous tissue. The vessels composing the disease are mostly very small veins or capillaries, and they derive their vascular supply almost entirely from the vessels of the cutis—i. e., a lateral and superficial source; they may simply stain the integument in varying degrees of vascular intensity, or the proper texture of the skin may be destroyed, being replaced by nævous tissue.

It is necessary for practical purposes to observe other distinctions—namely, that cutaneous nævi may be flat, on a level with the surface of the skin, or may protrude beyond it; that they may be definitely circumscribed, or may be diffuse, or spotted about the surface of the skin; again, they may be pendulous. Here are examples of each of the different forms of cutaneous nævus that it is necessary to recognise for therapeutical purposes.

There can be no difficulty in the diagnosis of this species of nævus; the appearance of the staining of the skin is characteristic, though care is well bestowed in ascertaining the exact limits of the disease, and the degree to which the structure of the skin is implicated. The colour of these nævi may vary from the slightest blush, through tints of lividity and crimson up to the brightest scarlet.

Mixed nævi, as they are called, are by far the most common variety of the disease. The bulk of the growth, in this case, lies in the subcutaneous tissue, while more or less of it involves the skin, either replacing the latter or staining it in varying degrees of intensity. They derive their supply of blood both from the vessels of the skin and those of the subcutaneous tissue. These nævi, like the cutaneous, may be circumscribed; or if diffuse, they may either gradually and imperceptibly fade away and blend with the natural textures, or they may at their margins pass into spots and patches of nævous tissue, cropping up here and there on the surface of the skin, from out of the subcutaneous tissue.

Venous nævi, of whatever kind, are generally congenital, being usually noticed at birth as small specks, spots, or lumps, beginning to enlarge rapidly a week or two after birth. Their tendency is to continued increase for an uncertain time, and before the age of puberty to disappear. This is especially true of nævous *tumours* (that is, the subcutaneous and mixed nævi), as distinguished from the strictly cutaneous variety. Cutaneous nævi are too apt to remain throughout life as permanent disfigurements, though they may fade somewhat in the intensity of their colour. In support of my statement, I refer to the opinion of some who have had most experience in this disease, such as Guersant and Moreau. Mr. Syme also entertains and has expressed decided opinions in favour of this view. The rarity of any other form of venous nævus than the cutaneous in adults is testimony in favour of it; while one's personal experience and published records furnish many instances of spontaneous disappearance of nævi when the period of childhood has been passed. Moreau mentions three cases where nævi condemned to operation by Dupuytren recovered spontaneously. Guersant relates an instance where several nævi on an infant disappeared in the same way. Bouchet gives another such case.

Here is the child I showed to you before with this large subcutaneous nævus on the abdomen undergoing a spontaneous cure. The manner in which a spontaneous cure may come about is either by simple shrivelling, by inflammation, by ulceration of the surface, or by degeneration of the nævus into cysts. Deep subcutaneous nævi are, of all others, the most likely to undergo shrivelling and cystic degeneration—a process that has been well described by Mr. Coote.

The florid cutaneous nævi that entirely destroy and replace the natural texture of the skin not rarely ulcerate, either spontaneously or as the result of blows, or from the friction of the child's clothing. Thus the natural history, as we may term it, of nævous formations is to begin life with the individual, to grow actively for a time, and, having lived its term, to degenerate or wither, and disappear.

The period of infancy seems the most favourable to the development of these growths. There are, however, two other epochs when already existing nævi may start into active growth, or fresh nævi may form: these are the commencement of puberty in either sex, and the period of pregnancy in women. The nævi that appear at these times are generally small and cutaneous, and are likely to disappear with the peculiar constitutional condition that favoured their formation.

[The natural tendency of all nævi when left quite alone is to degenerate and disappear. Why then interfere by treatment? For many reasons, because it is an unsightly disfigurement, especially upon the face; it is liable to accidents; by the time it has disappeared much of the skin may be spoiled by puckering in of the cutaneous surface, and lastly, because nævi in infants are generally small, and easily, and safely cured.]

Before considering the subject of treatment, let me say a few words on the kind of nævi most likely to undergo a spontaneous cure, and that within a reasonable limit of time. These are the large, unwieldly, and turgid nævi that are usually found about the axilla, or in the fat of the breast and the surface of the abdomen, and especially the enormous mixed nævi that sometimes grow about the parotid and submaxillary regions. Here are drawings of two very large growths of this kind, involving the parotid and submaxillary regions. Both of these disappeared spontaneously. Here is a drawing of a little girl who has been under my care with a nævus, the size of a cricket-ball, over the left breast. This sloughed and separated during an attack of scarlatina without causing hemorrhage or any untoward symptom.

The early signs of the probable retrocession of the disease are, a cessation of growth, an increase in solidity if it be subcutaneous, and a loss of brilliancy of colour. When the process has fairly commenced, light-coloured and dead-white spots appear on the surface, looking like the cicatrices of some caustic application; while the skin at these points is often slightly dimpled down and puckered in. Again, the occurrence of spontaneous ulceration over a cutaneous nævus may be regarded as warranting the expectation of a natural cure.

I will not weary you with a categorical description of the different means of treatment at our disposal, but will mention them in connexion with the varieties of the disease to which they are severally applicable.

Arterial, that is, a pulsating nævus, I have never seen in a child, and I much doubt if you will ever meet with the disease. Having no personal experience to guide me, I will not venture to attempt to teach you from the scanty materials afforded by others.

Treatment of cutaneous nævi.—For the more exact description of the treatment suitable to these I will divide them into (*a*) the the prominent, (*b*) the flat (either of these may be circumscribed or diffuse), and (*c*) the pedunculated.

(*a*) *Prominent cutaneous nævi* are generally very florid, and are entirely composed of vascular tissue, the proper texture of the skin being destroyed; there is often a considerable thickness of the diseased tissue, though it does not extend subcutaneously. They may be destroyed by caustic potash, Vienna paste, the strongest nitric acid, or by ligature, since there is no object in saving the deeply stained and morbid integument. It is advisable to select some plan of treatment that will in one application completely destroy the growth. If the disease, therefore, be circumscribed, circular or oval in shape, and situated on any part of the surface, except some parts of the face, you will find the ligature a speedy and certain method of cure. If the growth be of moderate size, say not larger than a penny, two harelip needles may be passed beneath it at right angles to one another, their orifices of entrance and exit being just where the nævus tissue joins the true skin. Beneath these a well-waxed and strong ligature of twine may be tied, being drawn so tight that the surface of the nævus assumes a ghastly pallid hue. The ends of the needles being cut off, the whole may be covered with a piece of cotton wool, and allowed to remain until the needles and nævus fall off; this generally occurs at the end of a week. The nævus should not be pricked to let out the blood while the ligature is being tightened, since this to a certain extent relieves the strangulation and diminishes the efficacy of the treatment. Should the nævus be too large to be included in one ligature, a needle may be passed beneath the base carrying a double thread, and thus the growth may be strangulated by two knots, each surrounding half the tumour. If the growth is still larger it may be tied in quarters, or in as many parts as may effectually arrest the circulation therein. The use of one or more harelip pins passed beneath the nævus is, of course, to ensure that the thread, in cutting its way through the tissues, may pass beneath the growth. There is a method of treating this variety of nævus, devised, I believe, by Mr. Gay: it consists in passing harelip pins through the sound skin beneath the growth, cutting the growth entirely out, and then applying the twisted suture over the ends of the pins, thus at once closing the wound and arresting hemorrhage. It is a plan which may occasionally be suitable, but when I have tried it I have generally found that the shape of the wound left after excising a nævus is such as to prevent the edges being closely and evenly approximated.

Prominent cutaneous nævi, when irregular in shape or ill defined in their boundaries, are best attacked by nitric acid, as this can be applied with a glass brush to the very smallest spots of diseased tissue without injuring the surrounding skin. The acid used should be the strongest that is made: the kind that is called "anhydrous" is best. It should be painted on with a glass brush, in a thin layer, until all appearance of vascular tissue has disappeared. The brush should never be so much saturated with acid that a drop could fall from it on to the neighbouring parts. Some carbonate of soda should be at hand in case of accident, and to apply to the nævus when the cauterisation is complete. Vienna paste or caustic potash is used by many for this kind of nævus, but I find it difficult to adapt them to the irregular edges and outlying portions of these growths.

(*b*) *Flat cutaneous nævi*—that is, such as do not project beyond the surface and have no depth of tissue—may, if circumscribed, be treated by vaccination, by nitric acid, Vienna paste, or the actual cautery. I should, if the child were unvaccinated, either inoculate the nævus in several parts with vaccine matter, or surround it with a circle of punctures. Though a necessary proceeding for the child, vaccination is a very uncertain remedy for a nævus. If vaccination is inapplicable, nitric acid may be used. I prefer it to the other potential cauteries, as well as to the actual cautery. The latter I never use, on account of the smell it occasions, and the horror it excites in a mother's mind; while its power of destruction are in no way superior to those of the potential cauteries, and the inflammation it occasions is much greater.

The flat cutaneous nævi vary much in depth of structure and in intensity of colour; they are sometimes extremely superficial, and only affect the very surface of the cutis, and do not extend into its substance. They might be called erythematous nævi, since they stain the surface like a blush. They are often spread over large surfaces. Here is a painting of one that affected the whole of one upper extremity: it was drawn from a little girl aged seven, under Mr. Holmes's care. I have only seen the erythematous nævi on the back of the hand, on the arm, the neck, and the face; but I know of no reason why they should not occur on any part of the external surface. Fortunately, this kind of nævus is peculiarly liable to disappear with advancing growth. Should it remain, however, and by its position cause any serious blemish, the surface may be frequently painted with collodion, or a small portion of the discoloured skin may be painted with blistering fluid to try its effect. It is to such a nævus as this, but more vivid in colour, that tattooing may be employed—a plan introduced and recommended by M. Paul de Landau. The skin should be stretched tightly between the fingers and thumb to control the hemorrhage, and, a multitude of small punctures being made with a common sewing needle, finely powdered oxide of zinc, or calcined magnesia, may be rubbed in: the process being repeated until the skin has regained its natural colour. I have never myself put this in practice, though it seems to be a promising plan of treatment in suitable cases.

The diffuse and spotted cutaneous nævi are best treated by nitric acid dotted on to the diseased points of skin.

Very minute cutaneous nævi may be cured by tightly stretching the skin, incising the spot with a small crucial cut, and touching it with nitrate of silver. I have usually washed the part afterwards with iodide of potassium in solution, lest any oxide of silver should remain in the substance of the cutis. I dare say that this is a needless precaution.

Subcutaneous nævus may be either *(a)* circumscribed, or *(b)* diffuse. *(a)* In this form of the disease, when the entire growth is situated beneath the skin, it is important to preserve the latter uninjured, and for this reason the following remedies are suitable :—The injection of coagulating fluids, such as solutions of perchloride of iron or tannic acid ; the introduction of setons steeped in the perchloride solution ; breaking up and introducing probes armed with nitrate of silver ; or complete excision, may any of them be employed according to the size and situation of the nævus and the predilections of the surgeon. The most elegant method of cure is by the injection of solution of perchloride of iron, since it inflicts but a single puncture. It is difficult, however—first, to be certain that the fluid will flow into the nævus ; and secondly, if it flows, to regulate the quantity. It may be used where it is of great importance to avoid all trace of treatment. It should not be employed to nævi about the head, face, orbit, and neck, except under conditions to which I will hereafter allude.

Perchloride of iron may more certainly, though less elegantly, be introduced into a nævus by drawing through the growth setons steeped in the solution. The thread employed should be darning cotton, on account of its absorbent properties ; and the needle should be large in proportion to the thread, in order that the needle-hole in the cutis may allow the thread to pass through, without squeezing out the perchloride solution. In a circular nævus, three, four, or more of these threads may be passed through the centre of the growth so as to form the radii of a circle. In an oblong or irregular-shaped nævus they may be drawn through side by side half an inch apart. While passing the threads, one finger may be kept lightly over the aperture of exit, and over the track of the thread, so as to squeeze out as much of the solution as may be thought fit. These setons may remain in for six or seven days. I usually remove them the day week after their introduction, and find that by that time a little suppuration in their tracks has occurred. In this way I am in the habit of treating a large number of nævi on all parts of the body, the head and face included.

Subcutaneous incision, so as to break up the nævous tissue is a good method of treatment for subcutaneous nævi : breaking up, together with the introduction of nitrate of silver, is a better; it turns the nævus into an abscess, and is an excellent remedy for the large subcutaneous nevi that occur about the trunk. In this method the nævus is thoroughly stirred and cut up with a long tenotomy knife introduced through the sound skin at the side. If nitrate of silver be not employed, pressure should be applied by means of a pad and a bandage. If nitrate of silver be used, it should be introduced on probes at the point where the knife entered, and be passed into all parts of the growth until the latter has acquired a feeling of considerable solidity; a poultice may then be applied, and the growth may be left to suppurate.

Complete removal of a subcutaneous nævus is quite possible, and not more dangerous than the removal of any other tumour, provided always that the growth is circumscribed, and that the incisions are not carried into the diseased tissue. A growth thoroughly suitable for removal by the knife is rarely met with; and should such a nævus present itself it would be easily curable by injection, by nitrate of silver, or perchloride seton.

A diffuse subcutaneous nævus—i.e., one where a certain, or rather an uncertain, area of subcutaneous tissue is spotted about here and there with vascular growths—is unsatisfactory in the highest degree to treat. I would advise you in these cases to wait for a cure by nature, since it is far more likely to occur through lapse of time than by your efforts.

Mixed nævi, as well as being the most common forms of the disease, are rather more difficult to cure than the former varieties. This is apparently because they receive their blood from so many sources, the deeper as well as the cutaneous vessels being channels of supply. These nævi rarely have any definite capsule like the completely subcutaneous variety. The disease is situated in the subcutaneous tissue, and projects through the skin. Like the cutaneous and subcutaneous, they may be either diffuse or circumscribed, and just as in these, the circumscribed are more easy of cure than the diffuse. Since they combine the principal features of the cutaneous and subcutaneous, they may require a somewhat complicated treatment.

When *circumscribed*, and but a small portion of their substance lies in the subcutaneous tissue, they can be treated conveniently by ligature, either in one mass, in halves, or in quarters, just as the prominent cutaneous nævi. Should the disease extend deeply beneath the skin, the subcutaneous part must be treated just as a true subcutaneous nævus. It may be broken up with a narrow knife, and pressure may be applied, or nitrate of silver introduced; it may be injected with solution of tannic acid or perchloride of iron, or the perchloride may be introduced on setons. After the cure of the subcutaneous portion of the growth, should the cutaneous part not disappear, it may be cured by painting with the strongest nitric acid.

This kind of nævus may grow to an enormous size, especially in the parotid and submaxillary region.

A little boy, aged 13 months, came here three years ago with a mixed nævus, situated chiefly in the parotid region, involving the pinna of the ear and meatus, and extending widely over the face towards the occiput, far down the neck, and along the submaxillary region towards the chin. I treated the most prominent and disfiguring portions of the cutaneous part of the growth with nitric acid, and did not meddle with the massive subcutaneous part. The disease has now disappeared.

There is a drawing of a similar growth in the same position in the museum of St. Bartholomew's. This disappeared spontaneously.

In 1864 a child was brought to me with a large mixed nævus, as big as a man's fist, in the parotid and left submaxillary regions. I passed two setons steeped in perchloride of iron through its deepest parts. No other treatment was employed. I believe that in this case also a cure has resulted, more, however, from the lapse of time than as the effect of the treatment employed.

Mixed nævi, if diffuse, are unsatisfactory to treat. The cutaneous part may be destroyed by acid or other caustics; the subcutaneous portion is best left alone.

Pendulous nævi are rarely met with. They are very vascular, easily bleed, and generally hang by a slender stalk. I have had but one under my care. It was just the size and colour of a large raspberry. It was ligatured at its base, and the root being touched with nitric acid, it completely disappeared. Ignorant people usually connect the most extraordinary tales with these growths, which are generally supposed to increase and bleed about the time of the ripening of the fruit they most resemble.

Peculiarities of treatment necessitated by certain localities.—There are certain parts of the body where the general rules applicable to the treatment of the different kinds of nævus must be modified so as to suit the peculiar requirements of the part. I shall now refer to those parts where special treatment is either advisable or necessary.

The scalp and forehead.—Here nævi may be sometimes advantageously treated by pressure constantly maintained by means of a piece of lead, or a small coin, stitched into an elastic silk band, or by means of any other device for exercising pressure that may suggest itself to the surgeon. It is a somewhat wearisome means of cure, and can only be successfully carried out with the assistance of an intelligent mother or nurse, who must be able to readjust the apparatus when displaced.

Nævi over the fontanelle should be more cautiously treated than elsewhere—*i.e.*, injection or perchloride setons, if used, should not penetrate very deeply. I have treated many in this situation successfully, but should always prefer to wait until the fontanelle be either quite closed, or at all events until ossification had taken place beneath the nævus.

The only case of nævus that has died under my hands, in consequence of an operation, was one situated in this region, where death occurred from sloughing after the introduction of perchloride setons.

Face.—It is well that you should be reminded that the treatment of nævi in this region by the injection of perchloride of iron has more than once been followed by instant death.

In the *London Medical Gazette* of 1837 the following case is reported by Mr. Paget, of Leicester :—A child, aged 2 years, suffered from a mixed nævus, as big as half an orange, over the right upper jaw. The growth was principally subcutaneous. On two occasions the nævus was pierced with a lancet, and a mixture of one drop of nitric ether with fifteen of nitric acid was injected. As this produced no effect, on the next occasion a small quantity of liquor ammoniæ was injected, the solution being so diluted that it could be smelt without its pungency being disagreeable. The instant that this fluid was injected, the child had one slight convulsion and was dead.

Mr. R. B. Carter, in 1863, reported the following case :—

An infant seven weeks old had on the nose a mixed nævus that was rapidly growing. The hot needle was employed without effect ; contractile collodion was painted on without avail. When the child was eleven weeks old a small quantity of perchloride of iron was injected, and a few days afterwards this proceeding was repeated. On this latter occasion ten minims of the solution were put into the syringe, and five minims were injected with a jerk, as the piston of the syringe did not move freely. A small discoloured spot appeared on the nævus over the point of the syringe, the child shrieked out, had a single convulsion, and was dead.

Again, I quote another, and the only other, instance, so far as I know, of this catastrophe. The case is reported by Mr. R. B. Carter, being communicated to him by Mr. Nathaniel Crisp, who was good enough to correspond with me on the subject. Mr. James, of the Melbourne Hospital, where the accident occurred, also gave me an account of it. It is by far the most instructive case, as it gives one the clue to the cause of the sudden death in these cases. The circumstances were these :—

A child suffered from nævus of the cheek, into which perchloride of iron was injected, and death occurred in fifteen seconds. On examination, it was found that the point of the syringe had passed into a small branch of the transverse facial vein; the blood was found coagulated in this vein and in the venous system down to the right side of the heart, which was filled with a firm clot.

This case, scanty though it be in its details, is sufficient to confirm the suspicion that would arise in one's mind as to the cause of death in the two previous cases.

As bearing on this point, I ought to mention that an infant under my own care was seized with difficulty of breathing a day or two after injecting a subcutaneous nævus on the face with perchloride of iron. It died, and after death exhibited symptoms of what is termed lobular pneumonia—*i.e.*, the lung-tissue was consolidated and apparently inflamed in patches. It is not impossible, in this case, that a small quantity of the perchloride had got into the circulation—too little to cause general coagulation of the blood in the right side of the heart, but enough to produce embolism in the pulmonary capillaries.

In any case, sufficient is known of the effect of the possible admixture of perchloride of iron with the general circulation, from injecting nævi on the face, to justify us in rejecting it as a remedy for nævi in these parts, unless, by pressure or by the employment of some instrument, the circulation in the growth is controlled, at least for a time.

Eyelid.—There is an admirable instrument (the ring forceps) for grasping the eyelid and commanding the circulation of a nævus situated on this part. The growth, when thus surrounded, may be injected, treated by introduction of nitrate of silver, or in any other way thought desirable, though ligature is not well suited for nævus in this locality.

Conjunctiva.—The only advice I have to give concerning the treatment of nævus in this locality is to leave the growth alone if it affects the ocular surface of the membrane

The *lip* is often affected with nævus on its mucous edge. During the application of any remedy, the circulation can easily be controlled here with a hare lip instrument or the ring forceps. The growth should, if possible, be attacked from the mucous surface of the lip.

The *ear* and the *nose* are very unfavourable localities for the cure of nævus. When the disease extends through the entire thickness of the cartilages, as it frequently does, painting with strong nitric acid has given me the best results. Should the nævus be in the form of a tumour, it can be ligatured advantageously.

It is a question with some, when a child largely blotched over the face with a highly-coloured cutaneous nævus, whether the crimson stain of the disease should be exchanged for the dead-white mark of a caustic. I am in the habit of treating these nævi at their most highly-coloured parts with nitric acid, as I am certain that, in an adult, no mere disfigurement, apart from real deformity, is so great as a plum-coloured or purple irregular stain over one side of the face. The acid should be painted on the most deeply-stained parts, and especially about the circumference of the nævus. I can strongly recommend this plan from repeated experience of its efficacy, and the satisfactory results it produces.

Nævus on the mucous surfaces—such as the gums, the vagina, and the rectum—is generally of the cutaneous variety. It may be cured by the application of nitric acid. Those in the rectum often give much trouble, from their great tendency to bleed; and unless this symptom was present, I should be loth to interfere, on account of the contraction that might follow the application of caustic. However, when hemorrhage occurs, nitric acid may be applied freely, so that if possible one application may be sufficient.

You may have observed that, in describing the remedies for nævus, I have omitted any mention of the treatment by croton oil, tartar-emetic ointment or plaster, simple setons, circular incisions, and the introduction of needles. It is not that I have not tried these means; but that they have either proved inefficient in my hands, or that the severity of the remedy has not been proportionate to the promise of cure.—*Lancet, July* 20, *Aug.* 3 *and* 17, 1867, *pp.* 65, 124, 191.

ALIMENTARY CANAL.

68.—ON THE TREATMENT OF OBSTRUCTION OF THE BOWELS, AND UPON CONSTIPATION.

By Dr. THOMAS HEAD, Carlisle.

[In the first case, which we give as illustrative of Dr. Head's treatment, the patient had eaten a large quantity of unripe fruit, great abdominal pain followed, with obstinate constipation. The bowels had not been relieved by any treatment adopted. The enemata used were of gruel, and had never exceeded a pint in quantity.]

On consultation I suggested the use of an enema of three pints of warmed oil, which was reluctantly acceded to, in consequence of the previous enema being still retained. Dr. Todd made it a condition that I should superintend its administration, to which I readily consented; the whole of the oil was slowly and carefully thrown up into the bowels; the abdomen became enormously distended, and the stomach rejected everything taken—even the smallest quantity of fluid of any kind was rejected. The oil was retained in the bowels by means of pressure over the anus by a soft napkin, continued for more than an hour. In less than three hours, part of the oil returned with slight appearances of fæculent matter; but, within ten hours after the warmed oil had been injected, four or five copious evacuations were passed containing enormous quantities of scybalous fæces. During the operations upon the bowels, anodynes and champagne were given, and some refreshing sleep ensued. This young gentleman's health remained for some time in a precarious state, but was eventually completely restored. Some weeks after this attack a thickened condition of the ileo-cæcal valve, which, however, disappeared, in course of time, could be distinctly felt by a manual examination. The bowels remained rather torpid, but in other respects his health was gradually and eventually completely reinstated.

In cases of obstruction resulting from the lodgment of undigested articles of food, observation has led me to regard the lower portion of the ileum or the ileo-cæcal valve as the locality of the disease, and having seen such frequent instances of disappointment in the use of enemata of the ordinary quantity, I have been led to place my chief reliance in those of large volume. For constipation limited to a loaded state of the colon, injections possessing stimulating properties will generally prove successful; but in cases similar to that described, they will most frequently be found inefficacious. A pint enema with a few drops of laudanum may indeed with advantage be first injected, and upon it the oil in the quantity described; the oil ascends through the watery fluid and is thereby more certain to reach the seat of the disease.

Obstruction of the Bowels.—Failure of small Injections.—Successful Effects of copious Injections of Oil.—Recovery.—A poor woman, a patient in the Carlisle Dispensary, married, aged 33 years, became the subject of obstruction of the bowels. The physician under whose care she came had not seen her for two days, but had ordered a small treacle and water enema to be administered with the old pipe and bladder apparatus. In consequence of his absence I was asked to see the case. All the symptoms of obstruction of the bowels in a severe form were present; the internal medicines which had been prescribed had proved unavailing, and had caused much vomiting. I ordered hot fomentations with turpentine to be applied to the abdomen, and a large oleaginous enema to be administered with the syringe and O'Beirne tube; in the course of a few hours the bowels acted freely with great relief to her sufferings; the symptoms of peritonitis, the result of intestinal distention, rapidly abated, and she made a speedy recovery.

The next case presents points of much professional interest.

Obstruction of the Bowels of ten days' duration.—Recovery.—A gentleman, aged 56, extensively engaged in the corn trade, who had been in the habit of biting wheat and other grain to ascertain their quality, and to swallow the particles thus introduced into his mouth, was attacked with great pain in the bowels (especially on the right side, in the situation of the ileo-cæcal valve) accompanied with vomiting; the pulse was rapid and feeble. The treatment consisted at first of hot fomentations and turpentine to the abdomen, an enema of oil saponified with liquor potassæ and turpentine; moderate doses of calomel, opium, and creosote were given at short intervals. From these remedies no relief was procured. The pain having increased in the right side of the abdomen, ten or twelve leeches were applied; no improvement in the symptoms took place; large enemata of oxgall and warm water were repeatedly administered; no satisfactory results followed. Three pints of warmed oil were now administered as an enema, and retained by firm external pressure to the anus. The sickness became most distressing, followed by severe vomiting, and a large quantity of the clear oil that had been injected into the bowels was thrown off the stomach; this took place repeatedly for the next few days, with, however, an apparent diminution of the pain and other distressing symptoms. No evacuation from the bowels took place. Pills of concentrated oxgall with creosote were now repeatedly given, and a free evacuation of fæcal matter speedily followed, mixed with immense quantities of comminuted grain; the motions appeared devoid of bile. The stomach became less irritable, and the vomiting gradually subsided. A tablespoonful of recent oxgall was given every morning with a few drops of chloric æther, and was happily retained, and the bowels by these means were brought into free daily action. The obstruction of the bowels was of ten days' duration. The patient gradually recovered his health, and with the occasional use of artificial and natural Harrowgate water the bowels have been restored to a much more healthy state than they have been in for a long time previously.

Obstruction of the Bowels of twenty-one days' duration—Recovery.—It must not be supposed that all cases of intestinal obstruction are characterised with the urgent symptoms detailed in the preceding cases; on the contrary, it may be safely affirmed that in persons of advanced age the system acquires under every form of obstruction a power of endurance much greater than in younger persons suffering from the disease. The following is a good example of this condition.

A lady living in the vicinity of Edinburgh, aged 74, became the subject of intestinal obstruction, and, notwithstanding that all the ordinary remedies were unremittingly used for a period of three weeks, no fæcal evacuation was procured. At this date the late Dr. Allison saw the case, in consultation with Mr. Sanderson of Musselburgh, and as a last resource small and repeated doses of calomel and opium were prescribed; in three days ptyalism ensued, and on the day following free fæcal evacuations took place; the bowels now became so very much relaxed as to threaten fatal exhaustion; by the use of stimulants and mild nourishment a slow recovery was made, and the life of the old lady was prolonged for several years.

The practical conclusions to which these cases would lead are—that in the intussusception of infancy inflammatory action which occurs in the middle periods of life is not excited in the serous membrane; that the straining and the evacuations of pure mucus streaked with blood are points of the highest diagnostic value. In such cases the peristaltic action of the small intestines ought to be allayed, rather than excited by medicines given by the stomach; and oleaginous enemata and quicksilver are means of the most probable efficacy for restoring the displaced intestines to their healthy relations. Where obstructions depend on the presence of undigested aliment, the lodgment most frequently occurs at the commencement of the large, or near the termination of the small intestines. After the lower portion of the colon has been cleared out by the use of a strong enema of oil, saponified with a little liquor potassæ, combined with about an ounce of turpentine, without the general peristaltic functions of the bowels being restored, it becomes desirable to use large enemata of warmed oil, the lower portion of the colon having been previously filled with a small ordinary enema of warm water, to which a few drops of laudanum may be added to secure its retention. Experience has appeared to justify me in believing that this mode of treatment is calculated to accomplish relief more speedily and certainly than the kind of treatment ordinarily adopted.

In the obstruction of aged persons the tendency to inflammatory complications becomes lessened, and the treatment may, on that account, be both of a less energetic character, and be longer persevered in before either relief to the obstruction or a fatal issue occur. It may here be also noticed as a point of diagnostic value, that in obstruction of the bowels, the seat of the disease being near the small intestine, the symptoms more nearly resemble those present in strangulated hernia; while, when the larger bowels are obstructed by scybalous accumulations, there is much less irritability of the stomach, although in both great abdominal pain may be produced.—*St. Bartholomew's Hospital Reports, Vol. III*, 1867, *p.* 91.

69.—OBSERVATIONS ON CERTAIN SUBSTANCES LIABLE TO BE MISTAKEN FOR INTESTINAL WORMS.

By Dr. WM. FRAZER, Honorary Member of the Medico-Chirurgical Society of Montreal, &c.

Cells of Orange Pulp mistaken for Entozoa and for Hydatids.—Three examples of this curious mistake have occurred to me. The first was referred for investigation some years ago from the north of Ireland by a physician who wished to ascertain whether the strange substances his patient had passed from his bowels were hydatids, as their appearance had given rise to much uneasiness. When informed of their nature his reply was, "I beg to thank you for your kindness in examining the supposed hydatids, the patient has been using oranges, and you are perfectly correct."

The second case was submitted to me by Dr. Austen. A delicate child had got an active vermifuge under the idea of worms being the cause of her ill health, and undigested cells of orange pulp were discovered in the alvine discharges, these were alleged to be genuine intestinal worms, but doubts having arisen about these supposed ascarides, he wished to have them examined. Dr. Austen's own impressions were opposed to their being worms, and his note in reply to my letter was, "You are perfectly correct as to the supposed worms, the child had eaten an orange or two for several days consecutively, the matter is really rather ludicrous."

In the third instance, a young gentleman of delicate appearance was brought to me by Dr. Kirwan; he displayed some substances floating in a phial of water, which he had picked out of the evacuations, and believed he saw them *distinctly moving*, they were cell walls of orange pulp, the contents being quite digested. When told what they were, he admitted that he had eaten the fruit. He was suffering from impaired digestion, and complained of irregular action of the bowels.

Undigested Celery mistaken for Ascarides.—Some years since a gentleman brought me several long undigested fragments of celery stalk, chiefly consisting of the stringy vascular tissue, which he fancied were worms. He had seen some ascarides lumbricoides passed by a child, and was convinced he was infested by them. The microscope showed their vegetable nature and rendered their recognition easy. It is not unusual for persons who have feeble digestion to excrete unaltered vegetable substances consisting of indurated vascular tissues or of sclerogen, and even the cellular portion of vegetables may pass off undigested, and it must be admitted in excuse for this patient's error, that the fragments of celery he mistook for entozoa were not unlike semi-digested ascarides.

Plastic lymphy intestinal exudation mistaken for Tape-worm.—In this distressing case a physician of great promise was attacked with plastic lymphy exudation of the intestines ; he had suffered from griping abdominal pains and a train of nervous symptoms, when some fragments of lymphy matter being expelled, he was supposed to have tape-worm and advised to use drastic purgatives and anthelmintics. These had the immmediate effect of aggravating his symptoms, severely injuring him and increasing the number and size of the expelled masses. The specimens that I examined were of pure white colour, seldom exceeding half an inch in length and of irregular shape, consisting of granular substance which passed into fibrillation on the surface that appeared to have been attached to the intestine. The lymphy fragments continued to form at intervals for several weeks until he left Ireland for a protracted sea voyage, and soon after his departure he ceased to pass these plastic masses.

Larvæ of Diptera supposed to be Worms.—In two instances, of which I have preserved no notes, dead larvæ of some fly, were brought to me under the idea of their being intestinal worms; they were stated to have passed from the bowels, which was possible, though their presence might admit of other explanations.

Elastic-ligamentous Tissue supposed to be Tape-worm.—Some years ago a woman in the wards of the Hardwicke Hospital, under the charge of Sir D. J. Corrigan, fancied she was attacked with tape-worms. The alleged worms were submitted to me for microscopic examination, they proved to be a quantity of fragments of ligamentum nuchæ which she had eaten weeks previously; they lodged in the cœcum, causing distinct fulness in that region, and required repeated purging for their expulsion. This case was recorded by Sir D. J. Corrigan in one of the early numbers of the *Dublin Hospital Gazette*, and affords a good illustration of the value of microscopic examination in determining the nature of doubtful substances—*Medical Press and Circular*, *April* 17, 1867, *p.* 361.

70.—OPERATIONS FOR HEMORRHOIDS IN TWO FEMALES.

(From a report of some operations at King's College Hospital.)

In these cases Mr. Smith adopted the method usually followed by him—an old Dublin operation, but considerably modified by

Mr. Smith. The part to be removed is seized with a clamp, the under surface of which, *i.e.*, that next the anus, is covered with ivory, and the instrument is prevented from slipping by a screw. The hemorrhoid, grasped by the clamp, is then cut away, and the actual cautery energetically applied to the wound, the ivory preventing the conduction of the heat, and thus saving injury to the parts behind. The screw is then gradually loosened in case of hemorrhage, which, should it occur, can be easily stopped by retightening the screw. Mr. Smith remarked that this operation was much less painful and dangerous than that of ligaturing and allowing the part to mortify away; for, by that means, sometimes fatal results followed, or, at least, the recovery was more tedious, whereas, in the case just operated upon, the patients would be well and moving about in three or four days. He mentioned that he had never seen hemorrhage with the clamp operation in internal, but once or twice in external piles.—*British Medical Journal, Oct.* 12, 1867, *p.* 313.

ORGANS OF URINE AND GENERATION.

71.—CONSIDERATIONS SUGGESTED BY THE STUDY OF 100 CASES OF STONE IN THE BLADDER OF THE ADULT, RECENTLY OPERATED ON.

By Sir HENRY THOMPSON, Surgeon-Extraordinary to H.M. the King of the Belgians.

The following cases of stone in the bladder of adult patients are not selected in any way, but are those which my own practice has furnished during the last three or four years. No exception has been made, every case being recorded as it occurred; so that the last 100 adult patients on whom I have operated up to the time of my writing form the material for the remarks which I shall have the honour to submit to my professional brethren. I have taken these cases because all have been treated with the same instruments and on the same principle, and because the number is a simple one for per-centage calculations, while it is sufficiently large to warrant our dealing with it in relation to some important particulars in connexion with this malady.

First, let me call your attention to the fact that these are all *adult* cases, and for the most part adults of an advanced age. It is necessary to bear this in mind, because, as we all know, the mortality following the operation of lithotomy before puberty is very inconsiderable as compared with that which occurs in after-life. Hence when we learn the rate of mortality in any given series of cases which contains patients of all ages, we do not really obtain any information on the subject unless we are at least supplied also with the number of patients of an age below puberty included in the total.

Reasoning from some extended researches respecting the death-rate in lithotomy which I very carefully made a few years since, I may safely assume that the mortality following that operation in children is 1 in 14 or 15 cases; that in middle-life it is 1 in 6 or 7; and after sixty years of age, about 1 in 3 or 4 cases. These broad numerical statements will serve our purpose as standards of comparison in dealing with the series of facts to which I have now to request your attention.

Secondly, in regard to the 100 cases under consideration, I have this, also, to remark: not only are they adults and the majority of them old men, but, in point of eligibility and fitness for operation, they fall short, in my opinion, of the average of stone patients afforded by any single district. These cases have been furnished from all parts of Great Britain, and some few have come from distant parts of the world—for example, from India, the Cape, and Canada. For I may, perhaps, be permitted to say, that it has been observed that known experience on the part of an operator attracts a certain number of patients, in unusually bad condition and with large stones, who seek his aid as their last chance of recovery. This fact, complained of by operators at an early time, was never so influential as at present, from the numerous facilities for locomotion now existing; and the effect certainly tends in a very appreciable degree, to injure the results of practice, if these are to be judged solely by a numerical statement of deaths and recoveries without reference to the severity of the cases.

With these preliminary observations, I will now proceed to state that the following 100 cases have been operated on in about three years and a half; that I have not refused to operate in a single case which has applied to me during that period; that I have employed lithotrity in every instance in which I felt it was safe and prudent to apply it; and have performed lithotomy in the worst possible cases, however bad, rather than leave the patient to the certainty of a painful and lingering death. Further, that I shall submit a table of the cases, as an appendix, which I will not occupy your time by reading; and associated with each case is the name of some gentleman who witnessed it with me, either in consultation or otherwise. Lastly, that in almost every case the stone itself is placed on the table for your inspection.

The following facts respecting these cases may first be noticed. Of the 100 cases, 84, or about four-fifths of the total number, were operated on by lithotrity and 16 by lithotomy. The mean age of the 84 lithotrity cases was 62½ years; among them were no less than 21 cases of 70 years and upwards, 2 being upwards of 80 years of age. Among these 84 cases there were only 4 fatal cases. The lithotomy cases, 16 in number, had a mean age of $63\frac{1}{2}$ years. The youngest was 42 and the oldest 80 years. 6 were above 70 years of age, including one each of 77, 78, and 80 years. 6 cases were fatal. Of the entire 100 cases of operation upon unselected patients, having a mean age of $62\frac{2}{3}$ years, and submitted to either lithotrity or lithotomy, there were 90 recoveries and 10 deaths.

I think I may now venture to assume that these results are successful—perhaps as much so as any similar series of cases which have been authenticated as carefully and satisfactorily as this has been. And I am anxious to promote the habit of thus authenticating cases; not that I have any doubt whatever that any circumstance stated by me to be a fact would be for an instant called in question; but I feel very certain that any operator would prefer that statements of so much interest and importance as those must have which influence surgical practice generally should rest on authority which can be tested, especially as this is quite easy to accomplish, without in the least degree infringing the confidential relations which ought to exist between us and our private patients, and which should ever be strictly respected.

I shall now endeavour to embody the most important of the considerations which are suggested by the study of these cases in four propositions, and shall remark upon each as briefly as possible.

The first proposition is,—That lithotrity is the most successful operation for at least four-fifths of all cases of stone in the adult which come under the surgeon's notice at the present time; a statement which is more definitely expressed in the fact, that the rate of mortality for such cases in this series is barely 5 per cent.

The second proposition is,—That these successful results from lithotrity can only be obtained by performing the operation in accordance with certain rules which can be laid down.

The third proposition is,—That cases of calculus in which one of the two operations, lithotrity or lithotomy, ought not to be employed are exceedingly rare.

The fourth proposition is,—That by exercising an ordinary degree of vigilance for adult patients suffering from symptoms of urinary disorder, every case of calculus may be discovered in an early stage; may be successfully treated by lithotrity; and, consequently, that the operation of cutting for stone in the adult may be rendered obsolete, or applicable only for some very exceptional example which has been developed as the result of extreme neglect or ignorance.

The first proposition—namely, that lithotrity is the most successful operation for at least four-fifths of all the adult cases of stone which come under the surgeon's notice at the present day—is supported, I may say proved, by the 100 cases before us. A rather larger proportion, indeed, than four-fifths was dealt with in that series. This must be regarded as an immense advance on what has been in past time the practice in Great Britain. I doubt whether, if we were to take for any given year all the calculus cases occurring in adult patients in Great Britain, more than one-half would be found to have been submitted to the crushing operation. I think I may safely say that very nearly one-half of the adult cases are still operated on by the knife. At the same time it must not be denied that in the hands of surgeons who have not practically studied lithotrity with care, the cutting operation would probably still afford the best results. Yet how immense is the superiority of the crushing operation in practised hands. Four deaths in 84 cases of men averaging 62½ years. I am satisfied that this result is attainable only with considerable experience and very careful attention to all the minutiæ of such cases. Yet the difference is so great that if even a much smaller success only is achieved, it is still superior to any which the knife can accomplish.

The next proposition is that lithotrity can be thus successful only when it is performed on a definite system, in accordance with certain practical rules which experience has determined, and which can be laid down.

But as a necessary preliminary to a very brief consideration of this topic, I would remark that, in every case of stone in the bladder, it is a matter of the highest importance first to make a correct diagnosis of the size and nature of the body which is to be removed, as well as of the constitution and condition of the patient; so that a judicious choice of the appropriate operation may be ensured. I have often had occasion to remark, that more harm may be done, and has been done, by the employment of the two proceedings—lithotrity and lithotomy—without due discrimination, than if lithotomy only had been resorted to in every case.

In making the diagnosis, the first fact necessarily ascertained is the capacity of the urethra. It is not common to find it deficient for the purposes of modern instruments, although it was frequently so regarded when larger and clumsier lithotrites were employed; at most a slight congenital narrowing at or near the external meatus may require a trifling incision to allow the lithotrite to pass with facility.

The size and form of the stone are next to be determined. The kind of sound to be used is by no means a matter of indifference. I should have thought it unnecessary to say this did I not know from experience how many surgeons still use a sound having the form of an ordinary catheter. Such an instrument does not admit of much lateral movement in the cavity of the bladder; while rotation is impossible. Hence it often fails to detect a calculus. An exploring sound should have a short beak, which can be moved with facility in every direction, and which can, therefore, be easily reversed or made to dip behind the prostate into the bottom of the bladder. I have made an addition to this sound, which I have found extremely useful in enabling me to ascertain the size of a calculus without using a sliding blade, or having recourse to a lithotrite for the purpose of measuring. Provided a suitable manipulation is employed, this can be accomplished with a very near approach to accuracy. The mode of using it may be thus described.

The surgeon, having struck the stone moves the beak of the sound to the further end of it, and slightly taps its extremity. The sliding ferrule is then slipped down the stem till it touches the external meatus. He then slowly draws out the sound, making a series of little taps on the stone as he does so, until he reaches the near end of the stone, and the instrument ceases to elicit a tap, having passed over it. Meantime, keeping his eye on the sliding ferrule, he observes how much of the stem has issued from the urethral meatus during the progress of the taps. This sounding will furnish the length of the stone to one-eighth of an inch, as I have often demonstrated at the bedside in University College Hospital.

If a still more precise demonstration is required, a flat-bladed lithotrite may be introduced; the stone caught at least twice—if possible in two directions—and the measurements noted, and while grasped in the lithotrite, it should be moved about in the bladder gently, when, if another stone exists, it will be struck. This proceeding usually gives rise to more irritation than the method with the sound, but it produces results which are perhaps a little more precise. The note from the stone struck, and the condition of the urine, as well as the condition of the patient himself, will indicate with great accuracy whether the stone is an acid or an alkaline one—a compound of the urates or of the earthly phosphates. The oxalate of lime calculus is much more rare; unless its volume is small, the resistance it gives to the lithotrite will indicate its quality.

In discussing fully the question arising out of the constitution of the patient much time might be occupied, but it will be held to be undesirable to do so here. In few words, a quiet, unexcitable condition of the nervous system, with no extreme susceptibility to feverish attacks, are points in favour of his submitting safely to numerous sittings if the stone be large, and *vice versâ.* The fact of renal disease existing, in my experience, by no means contraindicates lithotrity. Several of the patients in my list had advanced kidney disease, and were passing much constitutional albumen.

Supposing that a uric-acid stone of about an inch and a half in the long diameter is met with, and all the conditions are tolerably favourable, there is no doubt that lithotrity may be performed with a good chance of success. I by no means affirm that such a stone of two inches in the long diameter should not be crushed, but, as a rule, I think lithotomy is the safer operation. The weight of the former will probably be about six to seven drachms. After the weight of an ounce is reached, we arrive at the category of large stones. If the stone is phosphatic, one of much larger size may be treated by lithotrity.

[Sir H. Thompson next expounds those maxims which are chiefly valuable in this practice.]

1st. If the patient has never had instruments passed into the urethra before, it is as well to use a large flexible bougie or two, on two or three occasions, to observe the result both on the bladder and on the general condition. If fever is easily aroused by so simple a procedure, the question of performing lithotrity must be carefully considered.

2nd. The first sitting should be an extremely short one, and should consist in seizing the stone not more than twice (with the fenestrated instrument, only when large), and merely breaking it into several portions. The patient should remain in bed, and be admonished to pass water for the next twenty-four or thirty-six hours only while lying on his back. During the first two or three sittings I always enjoin this rule, esteeming it to be one of considerable importance. I endeavour to prevent the exit of débris during this period, in order that the fragments may become a little water-worn and their edges less sharp, and that the urethra may recover from any slight degree of irritation which the transit of the lithotrite may have produced. The fragments and the urethra are mutually better adapted to each other on the second or third day after the operation than on the day itself. I regard this practice as one which conduces in no small degree to prevent a painful, if not a dangerous, degree of irritation.

3rd. I am satisfied that the lithotrite should rarely remain at any sitting more than two minutes in the bladder. One minute usually suffices for three or four efficient crushings. Of course this is only possible with an instrument the construction of which admits of instantaneous change from screw to sliding movements, and *vice versâ;* and, for successful lithotrity, I do not hesitate to say that this is absolutely essential. I believe no lithotrite fulfils the various conditions required for rapid and easy working so well as that now made for me by Weiss, of London, and in great measure from suggestions which have occurred to me in constant practice to meet varied exigencies as they have arisen. Besides the quality just referred to, this lithotrite can be turned in every direction with perfect ease, while it can be held more firmly, when so required, than any other instrument. Moreover, a small quantity of débris may be removed by it with facility and safety every time the instrument is withdrawn from the bladder. The flat-bladed lithotrite is unquestionably that which should be employed whenever it is possible to effect our object by its means, leaving the fenestrated instrument for exceptional use only.

4th. Not only by making the sitting short, but also by diminishing as much as possible the use of all instruments not absolutely necessary, we should avoid producing mechanical irritation of the bladder. For by far the greater number of these cases a simple flat-bladed lithotrite has sufficed for every operation, and no other instrument whatever has been necessary, excepting only an exploring sound; and I do not hesitate to affirm that very rarely indeed is any other required. Most unusual, even among these elderly patients, is it to observe any indication for the removal of débris by washing out—a process formerly regarded as an essential part of the proceedings which constitute lithotrity. The now well-known apparatus of Clover is occasionally valuable when the bladder, from exceptional circumstances, does not sufficiently expel its contents, and requires to be emptied of retained débris. In regard to the practice of drawing through the urethra rough fragments of large size, I can only speak with the strongest reprobation. Not only must I regard it as injurious and dangerous, but as always wholly unnecessary. In thus acting, the injury inflicted is, in all cases, a purely gratuitous one. For whenever a fragment in the bladder is seized, a single turn of the screw suffices to pulverise it, and the product is at once in a condition in which it is harmless as regards the urethra. The perfection of lithotrity consists in easily reducing the stone to powder, and in allowing Nature to act by those forces which she possesses, and in the great majority of cases successfully applies to the removal of gravel and small calculi. In a word, all the dangers and difficulties occurring to the patient in lithotrity may be broadly referred to one cause—viz., mechanical injury to the bladder and urethra. The sources of this injury are two. The stone itself, and the instrument employed to remove it. To avoid these dangers and difficulties it is simply necessary—1st. To use the utmost gentleness and tact in the use of the lithotrite, which must be of the most perfect kind, and to be as sparing in the employment of all mechanical appliances as possible. 2nd. So to use such appliances, and so to manage the patient, as to reduce to a minimum those conditions of the stone itself which render it irritating. When these indications are fulfilled to their utmost, lithotrity reaches its highest degree of perfection. Thus, we shall aim at keeping the patient almost entirely at rest while the fragments are large and sharp, and we should reduce them to the form of powder as soon as possible subsequently. Sometimes it happens that cystitis is set up as the result of crushing a stone into large fragments, which does not readily yield to ordinary treatment. I have often found that, contrary to the usual rule, the patient is greatly relieved by a fresh crushing, which reduces these large and sharp fragments to smaller pieces, so rendering them less irritating to the bladder. Sometimes, too, under these circumstances we may go further, and by means of the flat-bladed instrument, we may, as an exception to the general rule, remove a considerable quantity of the débris when it is no longer well tolerated by the bladder.

5th. Few matters are of more importance, as regards the ultimate well-doing of the patient and the credit of the operation, than the removal of the last fragment. When a case of lithotrity draws to its close, and the symptoms have greatly diminished, but are not quite absent, it is often difficult to determine whether the bladder has been absolutely emptied of stone or not. On sounding, let us suppose that nothing is discovered; nevertheless, the patient feels a little pain at the close of micturition, and this act is performed with too great urgency and frequency. It is difficult to discover whether this is due to a fragment still remaining in the bladder, or to an irritated and slightly inflamed condition of that viscus or of the prostate, resulting from the presence of the stone and of the means which have been employed to remove it. If it is the latter, repeated soundings will only increase or prolong the morbid condition; if it be a fragment, one or two more soundings may be necessary to discover and reduce it. The real cause must, however, be discovered; but time will not allow me to enter on a complete practical consideration of this important point. One hint respecting a plan which I have found useful in this state of doubt is the following. Supposing that, after one or two careful explorations with the sound and with the small short-beaked lithotrite, the blades of which are easily reversed, and which can thus be made to traverse easily the floor of the bladder, nothing is found, I prefer to give up all interference, and let the patient have a week or two's freedom from instruments; but during that time he should on two or three occasions take a rough drive in an omnibus or other jolting vehicle. I know nothing which will so certainly increase the irritation if it is due to a fragment, while the effect will probably be very slight if none is present.

I now come to the third proposition—namely, that cases of calculus in which one of the two operations, lithotrity or lithotomy, ought not to be performed are exceedingly rare. I have already said that I have not declined a single case during the period in which these 100 cases were operated upon. On reflection, I might perhaps have done wisely to decline one case—a patient for whom I performed lithotrity,—a fatal one; and, further, I might have exercised a worldly-wise and prudent policy had I declined some five or six more whom I submitted to lithotomy. But then I believe it to be a dereliction of sacred duty to deny the one single chance of life to a suffering brother, lest the statistical results of practice should be impaired. Not unfrequently have I consented with much reluctance to operate; but it has been difficult to withstand the entreaty of a poor fellow for whom death was imminent, and, who, knowing fully all the risks, begged hard that he might encounter them, as his last and only hope. This recently occurred with a patient who, after seven years of suffering, came to me from the Cape—a mere skeleton worn out by disease. I declined, but offered an appeal from my decision, in the form of a consultation with my friend Mr. Paget. The refusal was confirmed, and, in despair, the patient sent for a well-known physician, and begged to learn why he should be denied the only hope of life, for which he had at so much cost of time and of pain crossed half the globe. The result was that we consented. I cut him, and he recovered. Is it not, then, almost impossible to say there is no hope? And if there be a single chance, how difficult to deny it! It was among such cases as these—cases of advanced age and of large stones—that I met the 6 deaths among the 16 of my lithotomy cases.

The fourth proposition is—"That by exercising an ordinary degree of vigilance for adult patients suffering from symptoms of urinary disorder, every case of calculus may be discovered in an early stage; may be successfully treated by lithotrity; and, consequently, that the operation of cutting for stone in the adult may be rendered obselete, or applicable only for some very exceptional example which has been developed as the result of extreme neglect and ignorance."

It may seem a bold assertion, that every case of stone in the adult may be treated by lithotrity. Depend upon it, this will be the case, and that its realisation is simply a question of time. I suppose no one will deny that it is an easy thing to diagnose the presence of calculus in an early stage, while it is yet small, and easily amenable to the crushing process. Very rarely does a calculus reach the size of a bean without giving marked symptoms of its presence; enough to send the patient to his surgeon; enough for his surgeon to suspect its existence. At all events, with anything like ordinary intelligence on the part of the patient and on that of his attendant, no stone should reach the weight of 100 grains, or measure an inch in its longest diameter; in other words, arrive at the size of a filbert without discovery.

Now, by way of proof, I wish to call attention to the striking fact that among the 84 cases of lithotrity, *all such stones were successful cases.* Each one of the fatal cases were stones of much larger size, occurring in patients worn out not merely by age, but by the long existence of the calculus and its fearful consequences. I am entitled, therefore, to say that every calculus being discoverable with proper vigilance while it is still small, it is removable by proper means, with an almost certain prospect of success. I regard the operation of lithotomy, then, attractive as it is to the practical surgeon by its brilliancy, and by the qualities which it demands for its successful performance, as destined ere long to be unknown, or nearly so, in the cases of the adult. For myself, I should resign it, with all its glories and excitements, with a profound regret, if I may be so permitted to speak, yet with a conviction that it will, though decreasing in frequency year by year, yet last perhaps during this century. The limits of time do not permit me to enter upon that fertile and interesting topic, the mode of its performance. Suffice it to say, that almost all the cases here recorded have been cut on the median and medio-bilateral methods; opening up a subject which I should have been glad to discuss had it come within the limits of our time. My object here has been rather to illustrate and enforce the capabilities of lithotrity, and to establish for it if possible a higher place in the estimate of British surgeons than I think it has hitherto attained. It is with this end that I have brought these facts before you to day, believing that under nc other circumstances should I find so good an opportunity of submitting them to the consideration of the profession at large.—*Lancet, Oct.* 26 *and Nov.* 2, 1867, *pp.* 511, 549.

72.—TWO CASES OF LITHOTOMY BY A MODIFICATION OF THE OPERATION OF CELSUS.

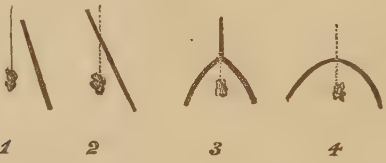
Under the care of Sir WILLIAM FERGUSSON, at King's College Hospital.

So content are surgeons ordinarily with the lateral operation for lithotomy that we do not very often have to record any departure from this well-known procedure. But the presence of a large stone in the bladder, and the want of sufficient space for its extraction which is occasionally experienced by operators using this method, every now and then tempt the surgeon into devising some means by which the difficulty may be remedied. Of late years the principal modifications have consisted of two—(1) the median operation of Allarton, and (2) the medio-bilateral of Civiale, which has been reintroduced and performed with very satisfactory results by Sir Henry Thompson (see Mirror, 1865, vol. ii., p. 673).

The procedure recently adopted by Sir William Fergusson is of peculiar interest because in the main it is a return to an operation of great antiquity, and which remained unchanged up to the sixteenth century. There is an important addition, however, to the proceeding. In the old operation, that described by Celsus (De Medicinâ, lib. vii., cap xxvi.), no staff was employed. A strong man seated took the patient on his lap and held the bent thighs apart. The operator introduced two fingers of his left hand into the anus and endeavoured to push the stone into the neck of the bladder, the right hand aiding by pressure above the pubes. A semilunar cut was then made (its horns directed to the ischial tuberosities) upon the projection formed by the stone in the perineum. The neck of the bladder was divided by a second transverse cut, and the stone removed by the finger or a spoon. In this process, which was called "cutting on the gripe," owing to the absence of a staff, there was always the danger that the urethra might be cut through transversely.

In the plan lately adopted by Sir William Fergusson, the ordinary curved staff was introduced into the bladder, and held as if for lateral lithotomy. The operator then plunged his knife into the right side of the patient's perineum, nearly an inch below the anus, and about midway between that aperture and the right ischial tuberosity. The knife then—its cutting edge being upwards—was carried over the anus and down again on the other side to a position corresponding to that where it commenced. In this way, a lunated cut was made, the concavity of which was directed vertically downwards. The forefinger and middle finger of the left hand, with their dorsal surface looking up, were then directed horizontally forwards, so as at once to push back the rectum out of harm's way, and to embrace between them the bend of the staff, furnishing thus an effective guide to its groove, which the knife then entered and completed the operation exactly as if for lateral lithotomy. The difference, indeed, between the process here employed and the usual one lay in the outer cut, which, instead of being directed downwards and outwards from the raphé of the perineum, formed a crescent which included the anus. The position of the staff, the deep incision, and the use of the forceps, were the same as those ordinarily employed.

It was on February 16th that a healthy-looking boy, aged five years, who had shown symptoms of stone for two years, was cut in this manner; and a lithic-acid calculus, the size of a large bean, extracted by means of the scoop. On that occasion Sir William, in referring to the procedure, illustrated his remarks by some diagrams upon the board, which we reproduce, as, even in the accompanying rough form, they will save much lengthy description.



1. The ordinary incision in lateral lithotomy.
2. The incision often employed by Sir W. Fergusson, which passes from the right of the raphé to the left ischio-rectal fossa.
3. An inverted ⋏ incision, which he had more than once employed with the object of gaining space.
4. The incision adopted in the present instance.

On June 1st we witnessed another trial of this procedure. The patient was a muscular man who had suffered for four years from symptoms of stone, and in whom lithotrity had been attempted in the country, but without success. Chloroform was administered, but the man was not bound. For a length of time past Sir William has ceased to bind patients whom he cuts for stone, and has rarely seen any inconvenience arise from the omission. But here the incision had only been half made when the man convulsively stretched out his legs, completely overpowering the assistants who endeavoured to restrain his limbs. Apparently not disturbed in the least by the novelty of the position which his patient thus occupied, Sir William, wihout halting for an instant, completed his incision with the utmost facility. The limbs then became sufficiently relaxed to be again brought into position, the succeeding steps of the operation were performed, and a good-sized stone was extracted.

In some interesting remarks which followed, Sir William said that he had long entertained the idea of adopting the process which his hearers had just witnessed; but it was only a few months since that he had first used it. The operation was partly the old one, "cutting on the gripe," described by Celsus, but employed long before him. It had gone out since the celebrated Frére Jacques introduced the grooved staff. At first Frére Jacques, ignorant of anatomy, had plunged a pointed knife into the perineum in the dark, as it were, and opened the bladder with varying success. Later he studied anatomy, and was understood to have been the first to use the staff, the employment of which, however, had been perfected by Cheselden, since whose time the lateral operation had been the one usually performed. Sir William remarked that the use of a staff quite altered the aspect of the old process, "cutting on the gripe." In its performance the knife was passed on and on, whilst two fingers in the rectum pushed the stone towards the operator. The uncertainty of the procedure and its danger put it out of date when the lateral operation was performed by Cheselden and his successors. The especial advantage of the lunated incision was that the cut was formed in exactly the widest part of the perineum, and it was, of course, of great moment to secure the widest space. It was an imitation of the direction and situation of Nature's outlet in the other sex—the vagina,—as the median operation might be considered an imitation of the direction of the vulva. Dupuytren, he said, had used a double bistourie caché, but his example had not been much followed. Indeed Paris did not now take the lead in lithotomy as it had once done. Probably lithotrity was more often performed there. Sir William thought that the combination of a lunated cut and the use of the staff should be fairly tested. As regards his first case, the patient had done well. The wound, however, was rather long in healing. He could not venture to say that this was on account of its shape. He pointed out the advantage gained by being able to introduce two fingers between the rectum and the staff, pushing back the rectum out of reach of the knife, whilst at the same time this very act opened the wound. In median lithotomy, pressure backwards of the rectum tends to close the wound. No doubt the rectum was not now so often wounded as it was thirty or forty years ago; but it was still very frequently cut, more often indeed, he thought, than was generally understood. In the present instance he had got into the bladder with as much facility as usual. He did not make a very free cut into the neck of the bladder, because he found that the stone was not so large as he expected. On touching it with the point of his left forefinger, he ascertained that a moderate opening for the forceps would be sufficient.

The patient made an excellent recovery, and was discharged on July 3rd.—*Lancet, July* 13, 1867, *p*. 42.

73.—CASE OF STONE IN THE BLADDER: LITHOTOMY BY A NEW OPERATION.

Case under the care of Henry Lee, Esq., at St. George's Hospital.

Mr. Henry Lee recently performed and described a new operation, or rather a modification of the lateral operation for lithotomy. The patient was a boy three years of age, who had at one time had retention of urine, and afterwards passed his urine very frequently, and always with pain. The first time he was sounded, Mr. Lee felt a stone; but subsequently he and other surgeons had sounded without being able to detect anything in the bladder. A week ago, under chloroform, a calculus was again felt; and, as the symptoms continued unabated, the operation was determined upon.

After an ordinary grooved staff had been introduced, the operation performed consisted of a straight incision in the median line, extending a distance of a little more than the posterior half of the perineum between the scrotum and anus, stopping a couple of lines in front of the opening of the bowel. From this point, the incision through the skin was continued outward and backward so as to embrace about one-fourth of the circumference of the bowel, at the angle formed by these two portions of the incision. The scalpel was then introduced in the median line, with its back towards the rectum. It was then passed forward into the groove of the staff, guided by the finger in the rectum. As soon as it had entered the membranous portion of the urethra, it was withdrawn, and a curved bistoury with a projecting probe-point introduced. The probe-point was then made to slide along the groove into the bladder, the edge of the knife being held to the operator's right side, as in Buchanan's operation. The heel of the knife was then made to describe a portion of a circle corresponding to the external incision; while the point, while being withdrawn, was moved little from the median plane. Mr. Lee mentioned that in this way all the advantages of a free external incision were obtained, with a very small opening into the bladder. The plan had, moreover, the advantage of ensuring that the point of the knife had entered the bladder. It was he thought, an operation the simplest in conception, the easiest in execution, and the least liable to be followed by any untoward accident, of all the modifications of the lateral operations for lithotomy. The groove in the staff was reached with great certainty, being felt by the finger in the rectum. The rectum was secured from danger by the edge of the bistoury being directed laterally. But the greatest advantage, especially in operating upon children, he considered to be the certainty of the incision extending into the prostate gland. It was well known that accidents had occasionally occurred from the prostate not being incised. When this had happened, the finger introduced had sometimes pushed the prostate before it; and in this way the prostate had been detached from the membranous portion of the urethra, and pressed towards or into the bladder, so that a cavity was formed without the bladder having been opened.

In the present instance, a very small stone came away at once between the blades of the forceps, together with some very small fragments which had apparently lodged in the urethra. The stone was so small, that it was doubted at first whether it was sufficiently large to give the sensation experienced when the sound had been used. The cavity of the bladder was, however, carefully explored, and nothing further detected. The symptoms had all subsided on the fifth day after the operation.—*British Medical Journal, Oct.* 12, 1867, *p.* 313.

74.—ON A CASE OF EXTREME IRRITABILITY OF THE BLADDER.

Under the care of Sir H. Thompson, at University College Hospital.

A patient (a case of supposed stone in the bladder) came under Mr. Thompson's care some time ago, with a bladder so irritable that he could not retain his urine even for a few minutes at a time, passing it as often as twenty-four times in the daytime, sometimes two or three times in the course of a meal. Mental influences had considerable effect upon it; if the patient became at all excited, it came oftener. Yet there was no pain in the penis, no blood in the urine, and seldom any pain in the back. When sounded no stone could be felt, and with all this the urine was perfectly natural and healthy in every respect—in fact, no cause could be assigned for the complaint. The remedy, however, was more apparent, for belladonna, exhibited as suppository and given by the mouth, speedily improved his condition amazingly, but he is not yet well. A hint *apropros* of this case may not come amiss to some of our readers, who have, doubtless, been troubled with cases of unhealthy urine containing much mucus mixed, it may be, with pus—consequently apt to decompose, become ammoniacal, deposit its phosphates, and irritate still more the already irritated bladder. They may have tried all the recognised and time-honoured remedies, including Brodie's favourite, Pareira, and yet have done no good. Under such circumstances, let them not forget a remedy sometimes ordered by Mr. Thompson—a common field plant, *Alchemilla* *arvensis*, or parsley-piot, an infusion of which (one ounce to one pint) will often succeed where more pretentious remedies have failed.—*Medical Times and Gazette, July* 27, 1867, *p.* 92.

75.—ON A CASE OF SPASMODIC STRICTURE OF MANY YEARS DURATION.

By Campbell de Morgan, Esq., F.R.S., Surgeon to the Middlesex Hospital.

The frequency and duration of spasmodic stricture are still amongst the unsettled points of surgery. Some foreign surgeons deny altogether the existence of true spasmodic stricture. In England the doctrines of Hunter and Home have taken too deep root to permit of such scepticism. But we find that the extent to which spasm may be regarded as a cause of obstruction to the stream of urine is very differently estimated by those who fully recognise its existence. The following quotation from Mr. Henry Thompson's article on disease of the urinary organs in the "System of Surgery" gives the opinion of a surgeon of great experience, and represents the views of perhaps the majority:—

"It is extremely rare that any considerable narrowing of the urethra takes place as the result of pure spasm of the muscles surrounding the passage. Granted, however, the presence of organic narrowing, or of inflammation in the canal, and an undue action of the urethral muscles may be excited, so as still further to narrow it. There is no doubt that a slight degree of this action may be excited in any part of the passage. ......But it is important to remember that the distinguishing feature which marks the phenomena thus ascribed to irregular muscular contraction, and by which they are contrasted with those of organic stricture, is their transitory character."

In the correctness of the first part of this quotation most will agree, but I doubt whether the second can be received absolutely and without some reservation. Generally, no doubt, the state of spasmodic stricture is very variable, but it is not always so. One sees cases now and then, in which all the signs of a narrow stricture are present, which yield readily to the contact of a bougie, and return rapidly when its use is discontinued, but where after death the appearances of permanent stricture are too insignificant to account for the condition during life. Admitting that a trifling organic contraction is present, the symptoms must surely be attributed to a permanent spasmodic state of the urethra, or to what comes practically to the same thing, a spasmodic contraction occurring whenever the patient endeavours to pass water.

The following case is, I think, a remarkable illustration of the possibility of the occurrence of true permanent spasmodic stricture. The patient—himself a surgeon—gives so circumstantial an account of his symptoms that no doubt can exist as to the fact that he had for years all the signs of permanent organic stricture; the result would show that he had probably never had organic stricture at all.

This gentleman, a surgeon-major in the army, is now 50 years of age. He first noticed the symptoms of stricture as long ago as 1847, after an attack of gonorrhœa, during which the spasmodic difficulty in passing water was so great, and the pain attending the effort so severe, that he always had recourse to the hot hip-bath. When the gonorrhœa had passed off he found that the stream of water was smaller and spiral, and that a few drops of urine always remained in the urethra. He tried himself to pass a bougie, but it seems always used a small one, and failed to reach the bladder. From this time the stream very gradually but steadily diminished, and passed irregularly, sometimes to one side, sometimes to another. He did not, however, care to have advice, as the complaint did not cause him much annoyance.

While on duty in Burmah in 1852 he noticed that there was a constantly recurring gleety discharge, and he had at this time a swelled testicle.

On returning to England in 1859 the stream was about the size of a crow-quill, and it took a considerable time to empty the bladder. He now began to suffer much inconvenience. Exposure, especially to night air, caused very frequent micturition, with pain in the lower part of the abdomen and loins. If he resisted the urgent desire to pass water great irritation of the rectum came on. From this time he was never free from these symptoms. The time taken in passing water became more and more protracted. The stream was sometimes thready, never larger than a crow-quill; sometimes the water has come in drops only. Lately he has suffered from pain in the perineum, muco-purulent discharge and irritation at the orifice of the urethra. The discomfort from pain, irritation, and frequent micturition became so great that he determined to undergo regular treatment. He placed himself under my care fully prepared to undergo any operation, by splitting, or perineal secton, so long as he could be quite cured.

The account he gave was so positive and circumstantial that I did not doubt for a moment that there must be some organic stricture. The perineum had a natural feel, but there was tenderness in the scrotal part of the urethra.

Instead of at first passing a full-sized instrument as I always do, I introduced a No. 6 catheter. On passing it through the scrotal region he complained of soreness, but there was no obstruction. At about the beginning of the membranous part the further passage was stopped. There was not any particular pain at this part, and the resistance was not firm. Smaller and smaller instruments were tried, but none would pass this point. Satisfied that there must be some spasmodic contraction, but at the same time believing that there was organic stricture, I ordered a large dose of bromide of potassium to be taken overnight and on the morning of my next visit. I then found that he had had a quieter night, but the instrument would not pass. A third attempt some days afterwards was made, under the same circumstances and with the same result.

I found that there was always tenderness in the scrotal region; and, desirous of ascertaining whether the urethra was narrowed at this part, I now for the first time tried a full-sized catheter (No. 11). There was no obstruction, and I passed it down to the seat of stricture. On making a very slight pressure the obstruction gave way, and the instrument passed into the bladder without the slightest difficulty. The next time he passed water it came away in a full stream; after that it diminished a little, but there was no further difficulty. I passed a No. 11 for him without meeting with any obstruction two or three times afterwards, and sent him home perfectly well, with instructions to use a large catheter if ever he found a tendency to diminution of the stream.

There can be no doubt, I think, that we have here an instance of long-protracted spasmodic stricture, never disappearing, indeed scarcely varying, during some years. It cannot be imagined that an organic stricture of over ten years' standing would at once yield to the gentle pressure of a No. 11 catheter and give no further trouble, when it had resisted the attempts to pass smaller instruments. It is possible that the irritable point in the scrotal part of the urethra may have had to do with the more distant spasm. It is possible that, whatever the source of irritation, the bromide of potassium may have quieted it. This is, however, all conjectural; but there can be no doubt as to the spasmodic nature of the obstruction. The case may be exceptional in point of duration, but it is not the less instructive. —*Lancet, April* 13, 1867, *p.* 453.

76.—OPERATION FOR VARICOCELE.

From a report of practice at University College Hospital.

[The operation described below, as performed by Mr. MARSHALL, is ingenious.]

The enlarged vessels were carefully separated from the vas deferens, and a needle threaded with silver wire introduced between the two, the needle pushed back, and the ends of the wire cut so as to leave a loop at one side and a couple of projecting ends at the other. A similar process was gone through at the same spot, only in the second operation the loop was left at the side on which the ends were in the former case, and *vice versâ;* also, instead of passing behind the swollen vessels, it passed in front of them, the veins thus separating the parallel wires. The ends of the wires were then passed through the corresponding loops and drawn tight, so as to strangulate the veins. The free ends were then brought together over a roll of lint, for the protection of the skin. When ready for removal a little pushing causes the stout wires to separate at the point where they strangulate the vessels, so that each can be drawn out by its loop. Two, three, or four of these pairs of wires may be used, as the case may require, and the result is satisfactory in the extreme.—*Medical Times and Gazette, July* 27, 1867, *p.* 92.

DISEASES OF THE SKIN.

77.—ON THE USE OF IODIDE OF LEAD IN CUTANEOUS DISEASES.

By Dr. T. W. Belcher, M.A., Physician to the Dublin Dispensary for Diseases of the Skin.

[The use of iodide of lead in the treatment of porrigo was introduced to the profession in Ireland by Dr. Neligan, in an article published in the Dublin Quarterly Journal for August, 1848.]

I have lately had under my care several private cases, in which I have used iodide of lead externally with great advantage; and as neither in the last edition of *Pereira's Materia Medica*, nor in the exhaustive *Traité de Therapeutique et de Matière Médicale*, by Trousseau and Pidoux; nor even in Professor Macnamara's last edition of *Dr. Neligan's Materia Medica* is much to be found about this medicine, which has been more or less in use for thirty-six years, it may perhaps be useful to give an abstract of a few cases in which it was used externally.

Case 1.—In April last, Dr. Owens, of Kildare-street, asked me to see with him a patient of his, a gentleman advanced in life, who had long been tormented with what proved on inspection to be chronic eczema. The disease was apparent on various parts of the body, but particularly on the legs and thighs; the itching was intense, and many preparations had been tried with little benefit. At first I suggested that an ointment of subacetate of lead containing glycerine and chloroform should be applied to the disease; and I had great hopes that this would produce immediate relief, as I had known it to do in other cases which I had treated shortly before that date. However, it produced little, if any, relief in this case; and accordingly, on my suggestion, Dr. Owens agreed to try the iodide of lead, which was applied in the form of ointment, twelve grains to the ounce, with one drachm of glycerine, and forty minims of chloroform. This procured immediate relief from the intense and aggravated itching; and the patient, having the advantage of frequent supervision from Dr. Owens, gave the remedy fair play, so that shortly after, on my seeing him again in consultation, I was agreeably surprised at the result. The disease had in great part disappeared; we agreed to continue the treatment, adding to the ointment as much chloroform as the preparation would take up. A few days ago I was gratified to learn, from Dr. Owens, that the patient was quite well.

Of course he took constitutional remedies as well as using local ones. It is doubtful whether in this case the iodide of lead, *per se*, would have cured the disease; chloroform certainly would not; but the conjoined use of the two was most beneficial; and I have no hesitation in recommending a preparation, such as I have above described, to the more general use of my professional brethren.

Case 2.—I was called in to see a young lady who was subject to erythematous eruptions on the face, amounting frequently to erysipelas. At this time the attack was of the latter nature; the face was red and swollen; not painful and itching. Besides giving a purgative, I ordered an ointment of about the same strength as that above mentioned. Its good effect was almost immediate. In two or three days I saw her without a trace of the affection for which I had first visited her. This was always before that time constant to a greater or less degree; and had been more or less benefited by various local and constitutional medicines; but now it wholly disappeared, and has not since returned, so far as I know.

Case 3.—A young gentleman, an undergraduate of one of the colleges in Oxford, of excellent general health, given to open-air sports, and without any evidence of personal or hereditary syphilitic taint, consulted me a few months since for psoriasis, which he had on most parts of his body, save his face. I directed him to take Neligan's ioduretted solution of the iodide of potassium and arsenic, described on page 268 of my edition of his work on Diseases of the Skin; and to use locally Hebra's tincture, which I have described on page 114 of the same work, and also in a paper in the number of this Journal for May, 1865.

He was further directed to take vapour and tepid baths, and physical exercise. I have seen him several times since his first visit; and always with the evidences of marked improvement in his case. At length I stopped Hebra's tincture, and a similar preparation of rectified spirit, soft soap, oil of cade, and oil of lavender, which for a short time previously I had substituted for the former; and prescribed for him instead of them the following ointment:—"Iodide of lead, twenty grains; simple ointment, seven drachms; glycerine, one drachm."

He was directed to continue the constitutional treatment as before. In about a fortnight I perceived the disease to be greatly improved; in fact, the psoriasis may be said to have disappeared, so far as external appearances went; and the skin had quite regained, in most places, its natural colour and texture. I know the amelioration must be largely due to the use of the constitutional means above noted; but in no case treated throughout as this was at the outset (see above) have I seen the same speedy repair of the skin and its function as was so very remarkable in this instance.

I might mention several other private cases in which I tried it with quite as much benefit as in the above, but they would inconveniently extend the length of these observations. The ointment of the iodide of lead of the present *Pharmacopœia* (1867) I conceive to be very much too strong for such cases as I have above noticed. It contains sixty-two grains to the ounce; whereas from a fifth to a fourth of that quantity is quite sufficient, and more useful than the pharmacopœial strength. In the cases of porrigo, in which Dr. Neligan first used it, he put thirty grains to the ounce; and therefore we read with little surprise that in some cases "iodide of lead ointment excites a certain degree of inflammation." No such result follows the use of the weaker preparation; but the stronger is nevertheless useful in other conditions of the skin than those I have described.—*Dublin Quarterly Journal, Aug.* 1867, *p.* 85.

78.—OBSERVATIONS TENDING TO SHOW THE IDENTITY OF THE FUNGI OF FAVUS AND TINEA CIRCINATA.

By Dr. John M. Purser, A.B., Demonstrator of Anatomy in the Carmichael School of Medicine, Dublin.

In an interesting paper by Dr. M'Call Anderson, recently published, in which he attempts to prove the non-identity of the fungi which produce the diseases known as favus, tinea tonsurans, and pityriasis versicolor, the following statement occurs:—"Of the numerous instances on record of the transmission of favus and tinea tonsurans *from the lower animals* by contagion or inoculation, favus has always given rise to favus, and tinea tonsurans to tinea tonsurans." This conclusion is supported by several cases both from Dr. Anderson's own practice and from that of Bazin, Gerlach, Bärensprung and Köbner, and, so far as I know, at the time of the publication of the paper in question no instance of a contrary nature was extant. The following cases, therefore, in which favus appears to have produced tinea circinata (which Dr. Anderson and most other dermatologists believe to be identical, as far as the fungus is concerned, with tinea tonsurans and sycosis), both by accidental contagion and intentional inoculation, may be of some interest as bearing on the question, still unsettled, of the identity or non-identity of the fungi which occur in the different epiphytic diseases of man.

In October last I was called on to attend a family, four members of which, all adult females, were suffering from ordinary tinea circinata. The spots affected the hands, arms, and shoulders; were of various sizes; presented a centre of sound skin and a spreading furfuraceous edge; itched a good deal; and, in short, presented a typical example of ordinary ringworm, so much so that, contrary to my usual custom, I neglected to make any microscopic examination of the epidermic scales. The diseaae was recent, and yielded readily to treatment. A few weeks afterwards, when the spots were all well, or nearly so, on talking over the possible manner in which the disease might have been contracted, I was asked by one of the patients if it could have been taken from a cat, mentioning at the same time that one of her cats had had a spot on its paw, which she, thinking it was dirt of some kind sticking in the hairs, had tried to pick off but found that her attempts made the paw bleed. Shortly after this she remarked spots of ringworm on her own hands. On inspecting the cat I found that it had on its left fore-paw a small spot, from which the hairs were almost completely absent: this was covered by a raised crust of dry powdery consistence, and sulphur yellow colour, through which passed a few stunted hairs. There was no cupped appearance, but the naked-eye characters of the crust resembled much those of some varieties of favus. I learned further, that another cat living in the same house had had a similar patch on its nose, which was now well. I inquired as to the mice, but could get no reliable information. Microscopically, the crust from the cat's paw was seen to consist almost entirely of the spores and filaments of a fungus. The spores were large, many of them oval, and present in considerable quantity. The filaments were branched and abundant. These appearances were identical with those which I had seen in cases of undoubted favus; but mistrusting my own limited experience in a matter of such delicacy as the diagnosis of a fungus, I sent a portion of the crust to Dr. Tilbury Fox, whose opinion on such a subject no one will be inclined to question. He pronounced the fungus to be an admirable specimen of the *achorion schönleini*, the parasite of favus. In the meantime I had inoculated my own forearm with the fungus, by scraping off the superficial layer of epidermis, and laying on the denuded place some of the crust reduced to powder, and covering the whole with a piece of sticking plaster. The plaster was removed after a hundred and twenty hours, during the latter part of which time there was intense itching of the inoculated part. There then appeared a circular spot on the forearm, the shape and size of the plaster. The centre was covered by large, loose flakes of epidermis, separated probably in consequence of the retention of the perspiration. They adhered by only a few points to the subjacent parts, which consisted of skin, reddened and softened from absence of horny epidermis, but presenting no sign of eruption. Around this was a slightly raised and furfuraceous border, and outside this again a circle of inflamed skin covered with minute vesicles. After exposure to the air for some hours, the central scales separated; the skin beneath assumed a more healthy colour and appearance, while the spot continued to spread centrifugally by the formation of successive crops of vesicles. These vesicles were extremely small, of subglobular shape, formed with great rapidity, and dried up quickly, leaving a furfuraceous crust. There was some difference of opinion among the physicians to whom I showed the spot as to what the disease should be called; but those who carefully observed the healthy centre and the spreading edge thought that it most resembled herpes circinatus, while all agreed that it had not the least resemblance to favus. Part of the crust was found by the microscope to consist chiefly of epidermic scales, mixed with which were a few pus or exudation corpuscles, a considerable quantity of the mycelium of a fungus, and a few spores. A specimen was sent to Dr. Tilbury Fox, who stated that the mycelium presented the characters of *trichophyton* rather than *achorion;* that it differed from the latter in being smaller, less freely branched, and containing fewer granules. The spores were small and round, none large or oval. He further says, "You clearly have a condition which presents, microscopically, the characters of tinea circinata (herpes tonsurans), but which may be only an early stage of favus; but this is exactly what, I argue, arises." Dr. Fox was also so kind as to show specimens of both fungi at the Pathological Society of London. After some days, as the spot continued to spread with unaltered characters, and as the itching had become so severe as seriously to interfere with my comfort, the crusts were removed. They were never renewed. The patch spread but little after that. It gradually died away with considerable desquamation, leaving a brown, discoloured spot, which persisted for several weeks. I had intended to attempt re-inoculation from my arm to a cat or rabbit, but was prevented at the time. Subsequently a second inoculation was made on another part of my forearm, which produced a diseased patch, presenting the same appearances, both to the naked eye and microscopically, as the former, but before I could continue the experiment, circumstances occurred which obliged me to relinquish the entire investigation.

It is unnecessary here to dwell on the mere fact of the transmission of disease from the brute to man, an event which is universally allowed at present to be not only possible but of frequent occurrence; but, as I have already stated, in all the hitherto recorded cases, favus has produced favus, and tinea tonsurans tinea tonsurans, whereas here we have favus in the animal giving rise, by contagion, to tinea tonsurans in the human subject; and the fungus, which, when growing on the hairy skin of the cat, under what we may suppose favourable conditions, appeared as *achorion*, with large spores and well-developed mycelium, on being transplanted to the human skin, where the hair follicles were few, and moisture deficient, changing its form to that of *trichophyton*, which is considered by many to be only an imperfect development of *achorion*. A change of a somewhat similar kind, produced also by transplantation of the fungus from a hairy to an uncovered part, is mentioned by Dr. Fox; but in this instance both forms of disease occurred on the same patient, favus of the scalp giving rise to tinea circinata of the shoulders and neck.

It is not my intention to go over all the evidence which has been adduced by writers to show that the apparently different fungi met with on man are all varieties, or different stages of growth, of the same plant, the diversity in form being due to differences in the soil on and the conditions under which they grow. It is, indeed, difficult to understand the opposition which, in the face of such evidence, this view still receives, at a time when the limits of variation in form of animals and vegetables, produced by external circumstances, is acknowledged by naturalists to be so wide. When we consider the slight differences in conditions of growth, which, in experiments on the germination of fungi, have sufficed to produce very different forms, we should rather be surprised that the variations occurring in the transmission of parasitic fungi from one person to another are so rare. Thus we read that "Dutrochet took a solution of albumen, and examined it from time to time. At the expiration of a year nothing had occurred; he added some acid, and monilia was produced; conversely he added an alkali, and botrytis resulted. Thinking that the agency of the reagent might have had some special influence, instead of albumen he used fibrin, and, curiously enough, he obtained just the very opposite result. Fibrin and alkali grew monilia, and fibrin and acid botrytis." There is some reason to believe that if the history of the source of contagion in each case of parasitic skin disease could be made out, and if more care were given to the determination of the microscopic characters of the fungi, instances of variation in the latter would be found to be more frequent than is at present supposed.

In conclusion I have to notice a paper, lately published by Dr. Salisbury, in which he describes a disease very common in cats, produced by the growth of a fungus, and very readily spreading by contagion to human beings, giving rise in them to a disease very like ordinary ringworm, but differing from the latter so much as to lead Dr. Salisbury to call it "a newly discovered disease," and to give it a new name, derived from its origin, *trichosis felinis*. It affects principally young cats, rarely spreading to their elders. The milk with which the paws and face of kittens are often smeared is believed to furnish a suitable soil for the germination of the fungus. Of human beings children are chiefly attacked. In them the disease spreads in isolated patches to all parts of the surface, often in a few days time. The patches are of various size, circular or oval in shape, slightly elevated above the surface, red and covered with scales and little elevations, marking the position of the hair follicles. The colour of the patches is deeper, and the irritation and itching more severe than in ordinary *trichosis*. The fungi also differ. "In ordinary ringworm (*trichosis furfuracea*) the fungoid cause exists mostly in the spore state. The plant does not advance beyond its cell condition. Its growth seems to be confined simply to cell-multiplication by pullulation. In this disease (*trichosis felinis*) the plant cells multiply by pullulation, and these advance to the filamentous stage of growth." From the drawings of the fungi which Dr. Salisbury gives, the spores are seen to be oval, containing one or two nuclei, provided with buds or off-sets, or occurring in chains. The mycelium is branched, jointed, with cellular interruptions, and contains no granules or spores. These characters are unaltered by the passage of the fungus from the cat to the child. In this respect the cases differ materially from those detailed in the present paper.—*Dublin Quarterly Journal, Aug.* 1867, *p.* 66.

79.—THE PERMANGANATE OF POTASH IN THE TREATMENT OF CARBUNCLE.

By Dr. THAD. L. LEAVITT, Germantown, Pa.

[The beneficial results of the local use of permanganate of potash in the treatment of sloughing ulcers, phlegmonous erysipelas, and hospital gangrene, having been thoroughly tested in America during the late war, it follows naturally that like good results will probably ensue from its employment in the treatment of carbuncle.]

Mrs. R., aged about 60 years, was visited, during the absence from town of her family physician, and found suffering terribly from a carbuncle located upon the left shoulder-blade, just above the spine of the scapula, and occupying the supra-spinous fossa. Loss of sleep, constant pain, and a naturally nervous temperament combined, induced a mental disturbance almost amounting to delirium. The tumour was in its sixth day, with all the general accompaniments, of the size of a hen's egg, tumid, tense, and shining. A free crucial incision had been made two days before, but with no relief; dense areolar tissue, puffy granulations, and sanious oozings crowded the track of the knife, with no appearance of separation or healthy action. The pulse was quick and compressible, 110 beats in the minute; countenance anxious and expressive of great pain; bowels regular. A strong solution of the permanganate of potash (ʒss. to f. ℥j.) was immediately applied with a brush, and a dressing saturated with it, covered with oiled silk, placed upon the shoulder. Anodynes, beef-tea, milk-punch, tincture of the chloride of iron, and quinia were administered. The same evening, the patient was again seen, and expressed herself as feeling much relieved; pulse 98, and gaining in volume and elasticity. The next morning the dressing was removed, and already, although but twenty-five hours had elapsed, true pus had begun to form, the intense pain had subsided, and the patient to use her own language, declared it "a miracle;" the pain had vanished, the fever was gone; she had slept well, and felt some appetite for food. A few days longer the potash was continued; the slough separated, and the wound healed in the short space of one week.

Mr. C., aged 50 years, shoemaker; was visited July 30th, 1866. Had been sick three days, was found suffering intensely from a carbuncle, situated upon the abdomen just below the umbilicus, of the size of a large walnut, and involving the surrounding structures in an erysipelatous inflammation. Bowels constipated; high fever; pulse 120; heavy breath; tongue furred; anxious countenance; great restlessness and general uneasiness characterised his principal symptoms. Hop and laudanum poultices had been applied, but he had been gradually growing worse, and approaching the position described, the tumour increasing daily, the parts becoming more dense, and at last an ichorous pus exuded from several small openings. Mild purgation, after which supporting and stimulant treatment was instituted. A slight incision was made, and the permanganate applied, as in the previous case, the dressings being removed once in twenty-four hours. This case was seen seven days successively; the 13th August he returned to his work, the severity of the suffering having been arrested after the first application.

Mrs. A., aged about 49 years, having suffered a few days from a supposed furuncle, and the pain becoming intolerable, called in medical aid. There was found upon the inner face of the left thigh, just below the nates, a well-marked, though small, carbuncle; a very slight incision was made and the potash dressing used. No constitutional treatment at all was inaugurated; in three days all signs of carbuncle had disappeared and the line of incision was healing nicely.

The results in this case were mutually gratifying, from the fact that about six years ago the patient suffered from a series of carbuncles appearing in succession, along the spinal column, from the back of the neck to the region of the lumbar vertebræ, and, lasting all through the winter months, her dread and fear of similar suffering were very great. The permanganate of potash has been eminently successful with me in the treatment of chronic ulcers. The following case, of many years' duration and which had resisted all efforts, yielded to the remedial qualities of this preparation.

Arthur M., tavernkeeper, aged 45, had a chronic indurated ulcer, of sixteen years' standing, extending over the superior face of the right leg, about four inches below the tubercle of the tibia, and spreading backward on both sides to the malleoli, covering a surface of about twenty-eight square inches, deep and burrowing in some localities, and in others merely superficial; the whole leg and foot were much swollen and anasarcous, the toes merely protruding from a shapeless mass of flesh, closely resembling the foot of a young elephant. An ichorous discharge of a horribly offensive character, together with filthy dressings, augmented the destruction of the surrounding parts.

The advice of an eminent surgeon had been secured a few weeks previously, to the effect that but one alternative remained, amputation; and indeed, all appearances favoured such a decision. Proper abstinence, tincture of iron and good diet were directed. The local use of a strong solution of the permanganate of potash and judicious bandaging have already done so much for this case that, at the date of writing, the tenth application of the potash, six square inches, will more than cover the small amount of ulceration remaining, so rapid have been the healing process and the formation of firm, healthy tissue ; and, in a few days more, we can confidently prognosticate a complete cure.—*Amer. Jour. Med. Sciences.*—*Canada Medical Journal, April* 1867, *p.* 462.

80.—SULPHURET OF CALCIUM IN SCABIES.

Vleminckx uses in scabies the sulphuret in the following manner:—Quick lime, one pound; spring water, a sufficiency; sulphur, two pounds; water, twenty pounds. Boil to twelve pounds. The patient is placed in a tepid bath for half an hour; then, on leaving it, all the parts affected are rubbed with a piece of flannel dipped in this solution. The patient is then placed in a second tepid bath for another half hour. The same measures are repeated the next day, and the itch is conquered. Eczema may follow this mode of proceeding, and this complication may take several days to control.—*Lancet, Sep.* 21, 1867, *p.* 361.

81.—ITCH IN DELICATE SKINS.

Hebra uses the following compound:—Petroleum and spirit, of each one ounce; balsam of Peru, one fluid drachm; oil of rosemary and lavender, of each fifteen minims.—*Lancet, Sep.* 21, 1867, *p.* 361.

SYPHILITIC DISEASES.

82.—ON THE REPORT OF THE VENEREAL COMMISSION.

By **Holmes Coote**, Esq., Surgeon to St. Bartholomew's Hospital.

The Report of the Committee appointed by the Lords of the Admiralty to inquire into the best mode of treatment of the venereal disease, has at least had the good effect of clearing the subject of much that was ambiguous, theoretical, or irrelevant, and has left it open to yet further inquiry. A syphilitic virus is recognised ; the disease is pronounced as specific as small-pox. But here the resemblance must cease : there is no further analogy between the two diseases ; and I trust never again to see repeated those cruel experiments by which it was attempted to render the body insusceptible to the further action of the virus by repeated inoculations.

The weight of evidence went to prove that syphilis, under favouring circumstances, might be generated spontaneously. And permit me to ask, as regards this point, in what respect the history of spyhilis differs from that of any other known disease? Do we know the origin of small-pox, of measles, or of scarlatina? Can we explain the nature of the cholera poison? To say that syphilis may possibly have been derived from the horse, is a purely fanciful statement, which is unworthy of any scientific work.

It being, then, established, first, that there is such a disease; and secondly, that there is a specific virus,—the Committee next proceeds to the subject of primary sores; and here, in my opinion, is the first break-down.

Two species of sores are described—namely, first, the "simple local sore, the influence of which never extends beyond the inguinal glands, but is eminently contagious, though incapable of infecting the constitution. This is the most common, and prevails over all other varieties in a ratio of about 4 to 1:" secondly, the syphilitic sore, which, according to this Report, is not always indurated, as was the prevailing opinion some weeks ago, but which may be indurated throughout its entire course; or soft in its early stage, and subsequently indurated or soft throughout its entire course, but which, unlike the simple local sore, is followed by constitutional disease. The evidence, further, is conclusive that it is impossible to pronounce with certainty on the characters of a sore on its first appearance.

Now, I maintain that such conclusions as these are useless to us in a medical point of view, and may become mischievous to our patients. The theories about "induration" as characteristic of syphilis are gone. The soft syphilitic sore cannot be distinguished from the soft non-syphilitic sore, except by its progress and history. If, according to the Report, a rash come over the body, it is syphilitic; if not so, it is a simple sore. But the surgeon, who honestly entertains this belief, can only reply to his patient's anxious inquiries respecting the character of the disease, "I cannot tell you its nature until it has come to an end. If you suffer from constitutional symptoms, it is syphilitic; if not, it is non-syphilitic." The patient might reply, that he could have told the surgeon as much himself.

Such arguments remind me of those once entertained respecting the use of mercury. If the sore is cured by mercury, it is syphilitic; if not so, it is non-syphilitic.

But these arguments may become mischievous. Is there a sane surgeon living who, if his life depended on it, would undertake to guarantee a patient, the subject of any form of venereal sore whatever, that he should be free from the occurrence of constitutional infection? Should there be any one who really attaches importance to the matter of "induration," I could speedily produce evidence to bring him to a very different opinion. That some persons suffer constitutionally, and some do not, is a matter of general notoriety; but surely all individuals are not equally susceptible of general syphilitic infection.

According to this Report, the frequency of constitutional symptoms to primary sores of all kinds should be four to one. Taking the female venereal cases which are in constant succession under my care, I should pronounce the number at least one-half; and then, many who quit the hospital apparently well, and not generally infected, suffer from secondary symptoms afterwards. So also, among the men, secondary symptoms are common; but the induration of the base of the primary sore is of rare occurrence, and is scarcely ever seen in women.

I see no reason to change my opinion from that which has been already published and expressed—namely, that there is but one venereal virus, the action of which is usually chronic, and always ulcerative. The character of the sore thus produced depends, as in any other case, on the nature of the tissue on which it acts; and, *cæteris paribus*, the same tissue presents the same kind of venereal ulcers. These ulcers are not precisely similar in the male and female; nor in the former are they always the same, inasmuch as they vary in character just as often as the tissues composing the penis vary in their intimate structure.—*British Medical Journal, June* 22, 1867, *p.* 728.

83.—OIL OF SANDAL-WOOD IN GLEET.

Mr. BERKELEY HILL, of University College Hospital, has tried the oil of sandal-wood in nineteen cases of urethral discharge of some weeks' or months' duration. Its efficacy, though uncertain, is often very marked. In several cases, it arrested the discharge by the third day; and, when taken for seven days, prevented any further return. Of the nineteen cases, the oil was of marked benefit in thirteen. In six, no benefit resulted from its use; and in four of these, any dose, however small, produced nausea or disturbance of the stomach. In two of the six, the drug had no controlling effect even when taken in two-scruple doses twice daily.

Of the thirteen favourable cases, cubebs and copaiba had been employed previously in seven without any advantage. In three of the seven, the copaiba produced too much disturbance to be borne; so that from these it would seem that some stomachs tolerate oil of sandal-wood that cannot bear copaiba. In all the patients one injection, in some several, had been employed before the oil was prescribed.

In the nineteen cases, the dose most often tolerated was fifteen drops, taken three times a day, with a little liquor potassæ, in peppermint-water; but eight drops in two patients had the desired effect, while forty drops were taken by three patients at a dose without producing intolerance. In these cases all other treatment was laid aside while the oil was being given. The oil was generally taken a week before the patient was able to leave it off.

Mr. Hill has not employed oil of sandal-wood in the acute stage of gonorrhœa. In these cases, he gives alkalies in frequent doses, with or without purges and anodynes, as required.—*British Medical Journal, July* 6, 1867, *p.* 7.

84.—STARCH INJECTIONS IN GONORRHŒA.

Bismuth has of late been largely employed with a view of obtaining a deposit of the powder into the follicles of the urethra. M. Luc, a military surgeon, proposes a cheaper injection of the same character—viz., finely-powdered starch mixed with lukewarm water, so as to obtain a fluid of the substance of of cream, but thin enough to allow of injection. M. Luc has been very successful with this compound, either in painless discharges, or where the inflammatory stage was over.—*Lancet, Sept.* 21, 1867, *p.* 361.

85.—USE OF THE SPRAY PRODUCER IN SYPHILITIC ULCERATION OF THE THROAT.

Case under the care of Dr. Murchison, at the Middlesex Hospital.

[Speaking of Dr. Richardson's now common spray-producer, the writer observes:]

By means of this little instrument a fine spray of sulphurous acid was showered over the tonsils and palate of a man who was suffering much distress from syphilitic ulceration of the throat. The improvement which took place was very rapid. The smell and bad taste which had been a great source of annoyance ceased immediately, and the unhealthy aspect of the sores gave place quickly to signs of healing. The man expressed himself very strongly as to the marked relief which the sulphurous acid, thus administered, had given him, and on looking into his mouth, some days after the application, we found the throat all but healed. The spray, he told us in reply to a question, produced no smarting.—*Lancet, Oct.* 19, 1867, *p.* 486.

DISEASES OF THE EYE AND EAR.

86.—ON THE EXAMINATION OF THE EYE WITH THE OPHTHALMOSCOPE.

By Dr. Henry Wilson, Member of the Royal Irish Academy.

[All that is required for purposes of illumination with the ophthalmoscope is a steady broad flame. It need not be very large, or very brilliant; on the contrary, a subdued light is to be recommended as being more grateful to the patient, and more suitable for the recognition of finer details. A German reading lamp with the shade removed is the illuminator generally used by the writer of this paper.]

For ophthalmoscopic purposes it is well to have the glass chimneys faintly tinged with blue, so as to modify the light. The flame should occupy a position behind the examined eye, so that its rays shall not directly impinge on the eye. I prefer, as a rule, having the light in one given position—a little above and behind the middle of the patient's head; others, however, prefer the light behind and to the side of the patient's right ear; and this position is probably the best where the examiner uses his right eye only. All direct sunbeams or other light which might interfere with the rays going to or from the examined eye, must be avoided. It is, however, not necessary that daylight should be completely excluded; a small amount of diffused and modified daylight may be present, and will be found very convenient. Sunlight is also employed in ophthalmoscopy, but principally in warm climates, and where the facilities we possess in our cities cannot be had. It is admitted through a circular orifice in the window, shutter, or door, or reflected by a plain mirror into the darkened chamber through the open door or window.

The eye should always be first examined without dilating the pupil artificially, as there are various objections to the latter proceeding. When the patient sits with his back to the light for a few minutes in the darkened chamber, and regards some distant object, the pupil will generally be found sufficiently dilated naturally for an experienced examiner to see the fundus, and to ascertain, in a large number of instances, the cause of the impaired vision. If, however, the pupil remain too small, or it be desirable or necessary to obtain a view of the whole of the lens or of the fundus, it can be readily dilated at any time; for the purposes of teaching, demonstrating, or learning also, it is well to have the pupil dilated; the substance used for the purpose is belladonna or its alkaloid atropia, the latter being preferable on account both of its activity and its cleanliness; half a grain or a grain of sulphate of atropia to the ounce of distilled water will suffice to dilate the pupil in healthy eyes in about half an hour (the strength of the solution I generally employ in diseased conditions of the cornea, iris, or lens, is one grain to the drachm). Where much inconvenience may arise, or cosmetic effects demand it, the pupil may be again contracted by a solution of the extract of Calabar bean.

Prior to studying on the human eye it is advisable to practice ophthalmoscopic illumination of other objects. The beginner should take the plate representing the arteria centralis retina in Gray's Anatomy, place it upright in front of a lamp or candle, so that it shall be in the shade, and then practice illuminating it from various positions, with and without the interposition of the lens, and with each eye alternately. Rabbit's eyes afford an easy and a beautiful object for study; the fundus in these animals differs, however, from that of the human eye; the eyes of persons, who are completely and incurably blind from cerebral disease, may be selected by the beginner, with advantage to himself, and without inconvenience to the patient; highly myopic eyes should also be selected, as the fundus becomes very readily visible.

The following observations refer, in particular, to the examination of the reversed or aërial image with Liebriech's small ophthalmoscope. The patient should be comfortably seated beside or with his back to a table, on which the light is placed; he should hold his head fully erect in one fixed position, and not move it about from side to side. The examiner should occupy a seat a little higher than that of the patient, and have his eyes on a somewhat higher level than those of the patient; he should also fairly front the observed. The distance between the two faces must depend on the refractive condition and the power of accommodation of both eyes. When these are normal it will be about eighteen inches—a short and convenient distance. I myself prefer standing in front of the patient for short examinations, and having the patient's head slightly inclined backwards, with the light vertically above and a little behind it. When dexterity and facility in examining have been acquired, this method affords many advantages; it allows of greater control over the patient; the examiner can move more readily in all directions, practice oblique illumination, and examine a number of cases in succession more expeditiously.

All preliminaries being now arranged, and the left eye the one to be examined, the transparent media are to be observed by oblique illumination ; and when it has been ascertained that these are transparent, the mirror is to be applied into the angle formed by the eyebrow and nose on the left side somewhat in the same manner as an eyeglass; the central aperture in the mirror should then be opposite the observer's pupil; the handle of the instrument is to be held horizontal, its extremity being grasped somewhat in the same manner as a flute between the thumb below and the three first fingers above; the little finger should be free, project beyond the handle, and be held erect. The upper arm may be retained at the side, or project from and be at right angles with the body. While being kept close pressed into the angle between the nose and eyebrow, the mirror should be moved vertically by rolling the handle between the fingers and thumb, and horizontally by movements of the handle backwards and forwards until the light is made to fall on the eye under examination. It is most important to recollect that the patient and examiner must retain their original facing position.

The right or non-examining eye may be kept open so as to control the patient's movements and the proper position of the reflected light ; it should, however, be occasionally closed, so as to make sure that the left or examining eye is looking through the ophthalmoscope opening. When the patient is unsteady it is advisable, in the beginning of the examination, to place the open right hand on his head, so as to keep it in the erect position, and prevent its moving. The examination having proceeded thus far, and the eye being now fairly and steadily illumined, the observer should perceive a brilliant red glow behind the pupil, which will be fixed or transitory according as the illumination or position is properly maintained or not. The patient is now desired to regard (without moving his head) the point of the observer's erect little finger, and when he has done so the red brilliancy will assume a somewhat white character, which indicates that the optic nerve entrance is then in the exact visual line. While the patient keeps steadily regarding the point of the little finger the examiner takes one of the large convex lenses—two inch focus—between the finger and thumb of his right hand, and holds it about two inches in front of the eye in such a manner that the rays from the mirror shall traverse it before entering the examined eye ; the hand carrying the lens may be steadied by resting the little finger on the malar bone or brow of the patient. The iris should not enter into the formation of the visual field ; the lens must be moved to and fro until the image of that membrane and its pupillary margin disappear altogether, and nothing remains but the circular bright red field. These directions being followed, and the patient's and examiner's eyes being normal, the latter should perceive the image of the fundus. In regarding this image, its position between the lens and the observer should not be forgotten, and the latter should, therefore, not endeavour to look through the lens, but should accommodate or adjust his vision for a near distance, and suppress the vision of his right eye. The lens must be moved from or approximated to the observed eye, or moved on its axis until the greatest degree of distinctness in the image is obtained; it must also be held quite vertically (to the axis of the eye), as otherwise inaccuracies, deceptions, or distortions may arise, which would lead to erroneous conclusions.

The entrance of the optic nerve into the eye is not exactly in the optic axis, but a little below and to its inside, having the yellow spot external to and on a level a little above it; the patient's eye must, therefore, be turned inwards and a little upwards, in order to bring the optic disc into the visual line, and the point of the erect little finger affords the most convenient and generally the best object for fixation. When it is wished to observe the position of the yellow spot or its vicinity, the little finger is closed, and the index finger is raised close to the mirror for the patient to regard; in this manner each finger may be raised successively, and a prolonged and steady examination made of the most important portions of the fundus. The little finger may be armed with a bright polished metallic thimble, which will readily attract the attention of amblyopic persons.

In the examination of the right eye the proceeding should be similar to that just mentioned, except that the ophthalmoscope is to be held in the right hand and to the right eye, and the lens in the left hand. We should in all instances make a comparative examination of both eyes.

I would particularly recommend this mode of examination to all who have equal vision in both eyes; it affords great facilities; the optic papilla comes into view the moment the patient regards the point of the extended little finger, and the hand carrying the lens is never in the way. When from imperfect vision in one eye, or other cause, the examiner can only use one of his eyes, he should have the light placed beside and a little behind the patient on the side of the examining eye, and direct the patient to look inwards and a little upwards towards the examiner's ear; thus, if the right eye be the examining one, the light is to be placed on the patient's left side, no matter which eye is to be observed; and if the left be the examining eye, the light should be on the patient's right side; the ophthalmoscope is to be held by the hand on the same side as the observing eye, and the lens in the opposite hand; by this means the illuminating rays are not intercepted. Where the patient is quite blind, or from other cause cannot fix his vision on the little finger, or in children, the examination is more difficult; in the case of adults the individual's hand may be placed in the wished-for position, and the person desired to look towards it; in the case of children the parent or attendant should occupy the position the child is to regard, and attract its attention by speaking to it, or holding up a watch or other bright object. If we fail by these means to obtain a view of the optic disc, we must alter our position. While the patient retains his head erect, and looks straight in front, the observer moves to the temporal side of the eye under examination, and getting about a three-quarter view of the patient's face, illumines the fundus in that position, when he will, as a rule, be able, with some little difficulty, to obtain the desired view. Where the eyelids are not opened sufficiently wide, the upper lid may generally be raised by the two middle fingers of the hand carrying the lens, these being disengaged while the thumb and index finger hold the lens, and the little finger rests on the malar bone. It should be our object, first, to obtain as extensive and general a view of the fundus as possible, so that any isolated irregularity or lesion will present itself at once as contrasting with the surrounding parts; and we should endeavour to have the optic nerve forming the centre of the picture. A slightly concave, or even a plane mirror, and a con-convex lens of 1½″ focus will give the desired results. Subsequently the individual portions may be magnified by low-powered object lenses, such as those of 3″ or 4″ focus, or by magnifying the aërial image by a convex ocular lens inserted into the clip behind the mirror.

The beginner must guard against being led into error by the *reflections* which are caused by the cornea and the convex lens: these reflections are at all times more or less present, and are sometimes very embarrassing. One of these reflections is a small but exact image of the ophthalmoscope mirror, and is liable to be mistaken for the optic disc. It may, however, be distinguished from this by the dark spot in the centre, answering to the orifice in the mirror, by the absence of vessels and by its position; these reflections may generally be obviated by slight movements of the lens on its axis, and to and fro. Occasionally a thread or film of mucus adheres to the cornea, and simulates, at first sight, an opacity in the transparent media; when such occurs, it must be removed by gently rubbing the upper lid over the globe.

The student must not be discouraged by getting mere momentary and passing glimpses of the optic disc. It requires considerable practice before we are able to obtain a steady and prolonged view of the parts. When the disc comes into view, and then suddenly disappears, it is attributable to some movement or unsteadiness on the part of either examiner or patient—most frequently, I think, of the former. When in searching for the optic papilla, a vessel comes into view, it should be steadily regarded and traced backwards in the direction of its increasing thickness, until finally the papilla is found where the vessel appears to terminate.

As I have already explained, the convex object lens produces the inverted image; and in reference to this image Mr. Carter, in his excellent translation of Zander's *Augenspiegel*, or rather his new, improved, and annotated edition of that work, says:—"In order to understand clearly the effect of the inversion, it is well to take a piece of thin writing-paper, and to draw upon it a circle to represent the fundus of the left eye of a person placed opposite to the spectator. The circle may be surrounded by the
Su
letters Na Te, so arranged as to indicate the superior, in-
In
ferior, temporal, and nasal boundaries of the figure. To the nasal side of the centre a small circle may be drawn for the optic disc, with lines radiating chiefly towards the temporal side to represent the blood-vessels. The sketch, as it stands, will give an idea of the erect image, with its temporal side to the right of the spectator and the vessels passing to the right. If the piece of paper be turned bottom upwards, with the written side still towards the spectator, the ordinary conditions of the inverted image will be fulfilled, and the inversion of all parts will be complete."

The chromo-lithograph, copied partly from Jaeger's *Beiträge zur Pathologie des Auges*, gives an idea of what the student is to look for; it represents the inverted image of the fundus of a healthy left eye, of a dark-complexioned adult. Although emanating from a concave surface, the image, as seen by the monocular opthalmoscope, presents a flat appearance; at its centre is the optic papilla, contrasting markedly by its whitish colour with the surrounding parts, which are of a brilliant uniform red colour, traversed by fine dark-coloured lines, representing the retinal vessels. The red colour varies according to the individual; it may be a very pale or very dark red, or may contain a yellow or orange, or even a greyish tint. The fundus, as already stated, consists of the optic nerve, the retina, choroid, sclerotic, and vessels and nerves belonging to these structures.

The intra-ocular end of the nerve, termed *optic papilla*, or *optic disc*, is circular or nearly so, and, as contrasting with the surrounding surface, appears of a white colour; it is, however, of a soft cream colour or pinkish grey, and not unfrequently conveys the idea of relief; it forms the most prominent and important feature in the image; it may be either sharply demarcated from the surrounding fundus, or may be partially or completely surrounded by one or two circles indicating the sclerotic and choroidal openings—the former white, the latter dark coloured. When the choroid is abundantly pigmented close up to the optic nerve, there is no appearance of the white or sclerotic ring round the disc, but when it is not we look down upon the sclerotic opening, which appears as a whitish ring round the nerve. Occasionally we find at one side—generally the outside—of the disc a very dark or black semicircular line; this is simply an accumulation of pigment in the choroid at this situation, and is seen in perfectly healthy eyes. Starting from the centre of the disc we find the branches of the centralis retinæ artery and vein, the former are the smaller of the two sets, and light red in colour. As a rule the artery, after emerging from the porus opticus, divides into two main stems, one above and the other below; these subdivide and branch out into numerous small stems, which take a direction towards the temporal side, coursing somewhat circularly round the yellow spot towards the periphery of the retina. The course of the veins is similar, both sets of vessels avoiding the yellow spot; the veins are larger in calibre, darker in colour, and a little tortuous; in addition to these larger branches several minute ones may be seen ramifying on the disc. The vessels do not usually subdivide until after leaving the disc; they do not always pass through the nerve in one and the same opening, and one vessel is often seen to cross the other—sometimes the artery beneath, sometimes the vein. In the drawing the artery is seen to cross the vein above, and to pass beneath the vein on the lower part of the disc. Occasionally the disc presents a slightly mottled appearance, attributable to a number of small oval, dark, or bluish spots; this appearance is due to the anatomical arrangement; the intraocular portion of the nerve being almost transparent, we can see completely through it as far as the *lamina cribrosa*, which reflects the light very strongly; occasionally some of the nerve tubules continue transparent for some distance on the cranial side of the perforated plate, and we are thus enabled to look down into them beyond the cribriform plate; hence they appear as dark spots, while the lamina cribrosa itself appears as a glistening white surface. It is not uncommon to find in the centre of the disc a depression; this may be of greater or less magnitude and simulate the cupped optic nerve seen in glaucoma. This *physiological excavation*, as it is termed, is due to the circumstance of the nerve fibres bending round suddenly at right angles into the retina, leaving a space at the centre between them; it is seen as a bright white spot, generally at the outside (real) of the papilla, or it may appear as an extensive cup-like depression of the nerve. In this normal excavation, however, the margin of the depression never corresponds with the margin of the nerve disc, which it usually does in glaucomatic excavation. Another remarkable physiological phenomenon consists in the visible pulsation of the veins of the disc, which is not a very uncommon appearance during health; it is confined to the veins, and does not occur outside the optic disc. This *venous pulsation* is attributed to the temporarily increased intraocular pressure caused by the influx of blood brought to the eye by the arteries at each systole of the heart; an increase in volume occurs in the coats of the globe and compresses the vitreous humour, which on its part reacts and presses on the veins, they being the most yielding parts; a complete or partial momentary stoppage of the column of blood in the vein is thus produced, and as soon as the transient pressure is removed the vein expands and the arrested column passes rapidly on. Immediately after the pulsation at the wrist the vein may be seen to fill from its peripheral end, and then, after a moment to empty. This venous and normal pulse must not be confounded with an *arterial pulsation*, which only occurs during disease, when the pressure of the vitreous is continuous and greater than the lateral pressure exerted in the artery itself. This pulse cannot be easily mistaken, as it occurs synchronously with the systole of the heart; it appears as a rhythmical movement of the red column of blood, and as the vessel appears empty during the diastole of the heart. Both these pulses may be produced by pressing on the outside of the globe with one of the disengaged fingers of the hand carrying the lens. Slight pressure will, as a rule, suffice to produce the venous pulse; if the pressure be kept up steadily and firmly the venous pulse ceases, the optic papilla pales, the veins empty, and the arteries pulsate, and dimness of vision ensues. When on slight pressure being made the arterial pulse becomes apparent, it shows that the intraocular pressure is already abnormally great. The physiological explanation of the arterial pulse was first given by von Gräfe, who likewise drew attention to its great practical importance and significance in the diagnosis of glaucoma.

In shape the optic papilla is, as a rule, circular. Sometimes, however, it is oval from above downwards, and very rarely horizontally oval. Whenever it is seen to vary from the circular shape, we should be on our guard and satisfy ourselves that it is actually anatomically oval, for by a false position of the lens its shape may be distorted. In case the papilla appears oval in the reversed image, we should examine it by the direct method, and *vice versâ*, as a comparison of the results of both methods may lead to the recognition of a rare anomaly of refraction—astigmatism. Minute spots of pigment have, on rare occasions, been observed as congenital peculiarities on the optic papilla of normal eyes.

The *retina* cannot, as a rule, be distinctly seen during health; it is, if not perfectly transparent, at all events highly translucent, and we look through it on to the choroid. When the choroid is deeply coloured with abundant dark pigment, the retina may be indistinctly visible as a slightly greyish cobweb-like membrane; occasionally delicate, isolated lines, representing the nerve fibres, may be detected radiating towards the periphery. The presence of the retina may, however, always be recognized by means of the branches of the arteria and vena-centralis retina, which, after passing the disc, course towards the periphery in the nerve-fibre layer of the retina. These vessels are, as I have stated, distinguished by their size and colour. The arteries are small, and filled with bright red blood, while the veins are larger, somewhat more tortuous, and of a darker colour. The arteries appear sometimes as if transparent along the centre of the vessel, or, which is the same, as if the lateral walls were opaque; this is, however, solely owing to the reflection of light, in the direction of its incidence, from the most prominent part of the vessel (that towards the observer). The retinal vessels appear almost to stand out in relief, and are easily visible. When one of these comes into view, while the observer is looking for the optic disc, he should trace it backwards in the direction of its increasing thickness, or in the contrary direction to that in which its vessels are given off.

The *macula lutea*, or yellow spot of Sœmmering, is situated to the outside of the optic disc in the retina. Its position in the inverted image is indicated by the dark shading to the inside or left-hand side of the disc. This so-called yellow spot is only exceptionally to be distinctly recognised; its position may, however, be at all times easily seen. The patient should regard the image of the flame in the mirror, or the point of the erect index finger of the hand carrying the ophthalmoscope, the finger being close beside the mirror. The position of the yellow spot is characterised ophthalmoscopically by the absence of retinal vessels of any size, and by the dark colouration which is due to a more abundant pigmentation of the choroid at this than at other places. The fovea centralis is said to appear as a small, bright spot about the middle of this dark-coloured portion. Those wishing to study this part more particularly should employ a plane mirror, with a concave lens behind it, and get the observed to regard steadily the flame of the image in the mirror; there will then be formed an image of the flame, or part of it, on the fovea centralis.

The *choroid* membrane lies beneath the retina, and forms one of the most important features in the ophthalmoscopic image, as to it is due the peculiar red colour of the fundus ; in the examination we are, in fact, regarding not the retina but the choroid. The colour of the fundus depends on the illumination and on the pigmentation of the choroid and its epithelial layer ; it is a bright blood-red, with an admixture of yellowish brown due to the blood in the tunica vasculosa and the chorio capillaris ; and to the pigment in the stroma and in the epithelial layer ; in fair and blonde persons the amount of pigment in the cells is small, and the fundus appears of a light yellow red, whereas in dark-complexioned individuals, with brown or black irides, the pigment is abundant, and the fundus appears of a brown red. Dark, brown, or black spots, with irregular outlines, are of not uncommon occurrence in the fundus of healthy eyes ; they are simply accumulations of pigment corpuscles, and are most frequently seen at the outside of the optic nerve entrance. As a rule, the fundus presents a uniform colour, and a faintly granular or stippled appearance, in front of which the retinal vessels stand out in relief ; or the retina itself may be recognisable as a delicate bluish grey film in front of the dark choroid. Occasionally the internal (epithelial) layer of pigment is so scanty and diaphanous that we are able to see the large choroidal vessels through it ; these are always of a lighter colour, and are likewise broader than the retinal vessels ; they are likened to flattened ribands, of a yellowish or orange tinge ; they are are not sharply defined, take a winding course, anastomose, and divide frequently, and may be distinctly seen lying beneath the retina ; by fixing a large dark retinal vein, we can satisfy ourselves of the existence of a space between it and the choroidal vessel, and by means of the binocular ophthalmoscope this relation becomes very manifest. Occasionally the pigment is so abundant and dark in the meshes of the stroma between the vessels, and so scanty in the epithelial layer, that the choroid appears as if mapped out into irregular, angular, dark, island-like spaces, bounded by yellowish lines, the latter representing the large choroidal vessels. In healthy eyes the fundus should be perfectly clear and brilliant, without any haziness or indistinctness of its parts. At the entrance of the optic nerve into the globe the choroid appears sometimes as a dark circle or half circle around the nerve.

The *sclerotic*, although entering into the formation of the fundus, is but rarely visible during health, being concealed by the choroid ; it contributes, probably, in a slight degree, to the colouration of the ophthalmoscopic image. Occasionally a small circle of this membrane is exposed around the optic disc at the nerve entrance, and appears of a brilliant white, or tendinous aspect. As a result of pathological alterations, this membrane frequently becomes visible.—*Dublin Quarterly Journal, August* 1867, *p.* 25.

87.—THE OPHTHALMOSCOPE AND THE PHYSICIAN.

By J. Vose Solomon, Esq., Surgeon to the Birmingham and Midland Eye Hospital.

Since the fact has become generally known to physicians, that certain cerebral diseases afford pathological signs in the optic nerve, which are demonstrable by the ophthalmoscope and by no other means, an increased desire has arisen on the part of the medical profession to be able to focus the fundus oculi with sufficient facility and clearness to obtain a perfect view of its condition.

Hitherto, the elementary difficulties have been such as to deter many physicians and some oculists from giving that amount of persevering labour to the method as would be crowned by success. It is evidently, therefore, the duty of the profession who have acquired familiarity in the use of the ophthalmoscope to aid, by bridging over difficulties, in its more general application to the discovery of morbid phenomena.

Many students get so far with the instrument as to be able to reflect and maintain the light of the lamp upon the eye of the patient, to apply their own organ of vision to the aperture of the mirror, and to bring the convex lens to a proper distance in front of the observed eye, yet without obtaining a definite outline of the optic nerve entrance and its vessels. At this point, so often do they stick, that at last, after repeated trials, the instrument is put aside, an instance of the *non possumus.*

The bar to further progress in such cases has its origin in an inability on the part of the physician to relax the accommodation of his own eye to the precise degree that will permit of a distinct aerial image of the parts named being seen by him.

I find all impediment to focussing of the fundus to be at once overcome if the student suspends the power of accommodation of his right eye by the application of a four-grain solution of atropia (British Pharmacopœia) half an hour before making his opthalmoscopic examination.

This being done, and the iris of the patient fully dilated, he has only, after having observed the usual rules laid down in ophthalmic works, to gently move his head a little backward or forward, as the case may be, and a complete picture of the optic nerve and its surroundings starts into view.

The physician will do well to select a case of extreme short sight, atrophy of the optic nerve, or a light blue normal eye, for his first experiment, and continue his observations every day. By the time the action of the atropia has subsided, he will, if endowed with good vision and some manual tact, find himself master of a step in ophthalmic investigation which had hitherto been considered by him as insurmountable.—*British Medical Journal, June* 1, 1867, *p.* 628.

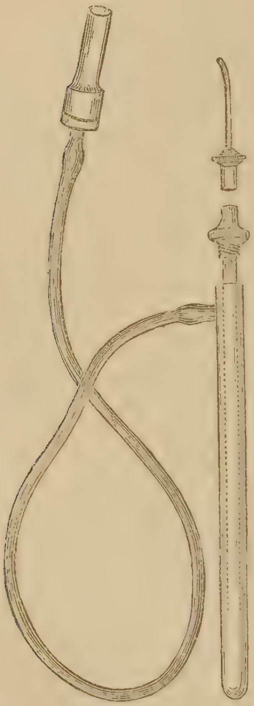
88.—ON AN IMPROVED SUCTION-TRACTOR.

By Henry Greenway, Esq., Plymouth.

The employment of suction in ophthalmic surgery has of late occupied the occasional attention of the profession. Some years since I published an account of an instrument I invented for the purpose of forming artificial pupil by the aid of suction, mentioning at the same time that the principle might be applied to other operations by merely altering the size and shape of certain parts of the instrument. The suction apparatus was composed of a plano-convex canula, having an aperture on the plane surface near its point, as an inlet for the iris. The near end of this canula was furnished with a screw and milled rim, whereby it was attached to a tubular handle. From this handle proceeded, at right angles, a short branch tube, on which was fastened an india-rubber tube, about fifteen inches in length, at the free end of which was a mouth-piece. Within the canula lay a blade for excising the portion of iris drawn in. This blade was acted on by pressing the end of a small rod which passed up through the handle.*

In 1864, Mr. Pridgin Teale, jun., of Leeds, proved that this very same combination of tubes, unfurnished with a blade, could be employed simply as a suction instrument for the removal of soft cataract, thus successfully reviving an operation which had become obsolete from the difficulty or danger attending the use of former instruments. Other suction instruments for this purpose have since been devised, Mr. Bowman's ingenious contrivance being one of them.

The following additions which I have made to my original instrument have been considered improvements:—Within the mouth-piece lies a valve, made of extremely thin india-rubber (oiled silk will not answer), which is opened with the slightest amount of suction, and is self-acting in closing. This prevents an accidental back current of air—a very important point when the eye is the part being operated on; for such accident has happened where there has been no valve. It also serves the purpose of a maintaining power for holding whatever has been sucked into, or laid hold by, the canula. The open end of the tubular handle receives a nozzle. In the inner opening of this nozzle is fixed a narrow tube, which extends nearly to the upper, closed, extremity of the handle. The chief use of this tube is to make the current more circuitous, and, consequently, the suction action more gentle. The outer opening of the nozzle receives the canula. Various sizes and shapes of canulæ can be fitted according to the requirements of the case. The one shown in the engraving is small, and suitable for extracting fluid—such as soft cataract, &c. I have another for laying hold of a hard cataract, in a case of linear extraction, where there is a difficulty in dislodging the lens from the capsule. Also, another having a very small india-rubber cup appended, whereby a foreign body lodged in the ear, nostril, or other cavity can be withdrawn.



This last idea has led me to propose, to a naval surgeon who has seen much service, the employment of an instrument for the extraction of balls from gunshot wounds by means of suction. My suggestion was received favourably, and I trust I may soon be enabled to lay before the profession the result of my experiments.

I have been asked, How is a surgeon to judge as to the amount of suction-power required for the removal of soft cataract? I believe no writer on the subject of extraction by suction has thought it worth while dwelling on that point; as it is generally supposed the operator would naturally be cautious in the employment of a force within the eyeball. If, however, any answer be required, I would suggest the following, which is somewhat in accordance with the rule for speaking in public: "Begin low; proceed slow." Commence sucking with the slightest effort possible; and if this should not suffice, then increase the power almost imperceptibly until the desired result is obtained. Should the canula become choked, which would be known by suction becoming difficult, it would be unsafe to increase the suction-power to a great extent; for the plug might suddenly be dislodged, thereby causing a rush of the iris or some other structure towards the aperture of the canula. In the event, therefore, of any plugging, the canula should be withdrawn from the eye, and the obstruction forcibly removed, as recommended by Teale and others; the operation to be then resumed.

In holding the suction instrument, the india-rubber tube, where it joins the handle, should be directed downwards; for by the observance of this caution any twisting motion of the instrument in the fingers is obviated, and the operator's hand is more at ease.

I think the term "suction-tractor" a suitable name for the instrument, whether it be used for drawing a fluid *through* it or a solid *on* it.

In the engraving (half-size of instrument) the nozzie is unscrewed and slightly withdrawn from the handle, thus exposing a portion of the inner tube, the remainder of which is indicated by the dotted lines. The canula is shown disconnected, from the nozzle. The tubular handle and the tube within it may be made either of glass or metal. The latter is to be preferred if the surgeon reside in a locality where a breakage could not be repaired.

The Messrs. Weiss supply the instrument, their name being a sufficient guarantee for good workmanship.—*Lancet, June* 8, 1867, *p.* 693.

89.—ON THE OPERATIONS FOR ARTIFICIAL PUPIL.

By HAYNES WALTON, Esq., Surgeon to the Central London Ophthalmic Hospital.

Should an artificial pupil be made in the one eye while the other is yet sound? My own opinion is against operating, as a rule, so long as the one eye is efficient, except the pupil can be made in the centre of the iris of the disabled or blind eye, the lens being present or absent; or upwards or downwards, the lens being present: for if the pupils do not have this correspondence, there will most probably be confusion of sight, double vision, or squint. A dissimilarity in the positions of the pupils is generally, but not always, followed by this disturbance of vision: why only occasionally I am unable to say; I only know the fact that, under apparently the same circumstances, when the pupils disagree, sometimes there will be the derangement, and sometimes not any. I recommend the operation, even although sight should not be altogether restored, but only a moderate amount of it acquired; because there is much gain, especially in the lateral use of the eye.

This is a subject that I have carefully examined in a practical manner, for theory can afford no help. Moreover, I am giving my own conclusions, unbiassed by those of others; for I am totally unaware of any published opinions, or of conclusions come to, by any observer in the same field. But I know that there is a general impression in the profession, and out of it, against interfering; and this seems to have come down traditionally from an age when ophthalmic subjects were but little understood.

When a person has lost an eye, he is not only blind on one side, but is reduced to a very limited range of vision for near objects beyond the centre of the face, and the angle of which is regulated by the prominence of the nose. The definition of sight, on which binocular vision depends, is lost. The power of accurately estimating distance is lost. Still worse than this, with labour requiring minute sight, there are more readily developed the many defects of impaired vision than when two eyes are used, because one eye cannot with impunity do the work of two. Therefore that measure is admissible which, while it restores the sight, does no harm elsewhere.

It can happen but as a rare opportunity that an artificial pupil can be made in the centre of the iris while the crystalline lens is present and transparent. Where this can be effected, the highest attainable perfection is necessarily gained. All the beautiful functions of the eyes are restored. When the pupil is lateral, even when the lens is present, the quality of sight is necessarily lessened. Nearly always, however, the lens is absent when a pupil is made, and minute sight must depend on the use of glasses of a high power, commonly called cataract glasses. The adjusting power of the eye is gone, and for seeing at different distances glasses of different foci are required. But, practically, an eye cannot be fitted with glasses and brought up to a state to match the other. Therefore arises this important question, on which the whole matter hinges: Will this kind of sight, which is inferior to that of the other eye, and at times in marked degrees, be really of material service? I say yes; I am satisfied about it; I act on the belief of it. I would be deterred from operating only by the probability of the eye being too much damaged to afford that amount of sight known as "useful." There can be no mistake about the results I have obtained. They are these:—Side blindness has been removed, and direct vision assisted. In those cases in which the lens was present there has been restoration of the ocular adjustment, even when the pupil was lateral.

I have been careful to test the statements of patients by my own observation. In casting my eye over the notes of the cases in which I have made a central pupil, the lens being absent, I find that pains and care were taken in jotting down the effects. In all the patients expressed their satisfaction and pleasure for what I had done. According to my experience, then, confusion of vision does not follow the restoration of the pupil when there is perfect vision in the other eye. This agrees with the fact that in "coloboma iridis" on one side, with the usual imperfection to vision that it causes, the other eye being sound, there is no confusion of sight; and that, when the defective eye is closed, much useful vision is cut off.

When two artificial pupils are made, the crystalline lenses being absent, both must be central. or that confusion of sight will ensue which will oblige one of the eyes to be closed. Several times persons with dissimilar pupils, suffering the annoyance, have called on me, told their trouble, and asked to have one of them closed.

When, after the loss of the crystalline lens, inflammation has destroyed the pupil, the iris does not necessarily alter its position. It does not bulge from the loss of communication between the chambers of the eye, provided the tonicity is preserved , and then the size of the anterior chamber is not only undiminished —a matter of importance in operating—but may be actually increased by the falling back of the iris, in consequence of the absence of the lens. There may be but a mere trace of the remains of the pupil; or the aperture, very much contracted, may be closed by lymph or capsule.

When prolapse of the iris through the cornea has shut up the pupil, there must always be a reduction in the size of the anterior chamber, the diminution depending upon the position at which the iris has escaped. The two states—closure of the pupil from inflammation, and prolapse of the iris—may coexist.

The operation of "incision," with the iris knife, is the most appropriate in any case where the iris retains enough of its physical properties to gape on being divided; and the less it has suffered from the effects of inflammation, the more certain will be the result. I invariably operate through the cornea. The following methods of steadying the eye are applicable to all these operations:—An assistant draws down the lower eyelid, resting his finger on the malar bone. I raise the upper eyelid with my forefinger, and with the tip of it and of that of the middle finger steady the eyeball. If in any case the steadying of the eyeball by the assistant by holding the conjunctiva with a pair of forceps be serviceable, it should be adopted. Sometimes I retract the eyelids with a spring-wire retractor, while the assistant steadies the eyeball with the forceps. This is more applicable when it is important to retain all that can be kept of the aqueous humour.

With a large or small knife, according to the circumstances ot the case, I divide the cornea as near to the circumference as practicable, and penetrate the iris as centrally as possible, thrusting the blade up to the shoulder. Except the knife be sharp and well used, the iris may be dragged on and torn from its ciliary attachment. The aperture thus made is about the third of the diameter of the iris, elliptical and vertical.

A great deal of stress is usually laid on the special fitness of "incision" in cases where the iris is on the stretch from prolapsus. This property is much overrated. The choice of the operation, so far as the state of the iris can be taken as a guide, should be made to depend on its actual structural condition; because any advantage that the mere stretching could afford might be lost by slight interstitial change, and inflammation of the eyeball is often a cause of the prolapse.

The pupil may be closed after the absorption of the lens in capsulo-lenticular cataract from the effects of inflammation prior to an operation or after it. "Incision" is suited when the iris will retract.

It was by "incision" performed through the sclerotica that Cheselden, our countryman, and surgeon to St. Thomas's Hospital, the originator of artificial pupil, used to operate. His method fell into disrepute and was almost abandoned, when Sir W. Adams revived it.

The great advantages of the operation I advocate are—its being executed through the cornea; the certainty of being able to make the pupil at the desired spot; the division of the iris before the aqueous humour is lost, and therefore while it is tense, by an incision which, owing to the form of the knife, is effected with such slight pressure that there is no risk of detachment from its natural connexions.

To render "incision" with the knife more generally applicable, by adapting it to cases in which the iris has not sufficient tone to contract when merely incised, and when it is more or less tied or fixed by being prolapsed, or adherent to capsule, I proposed some years ago a modification of the above operation—namely, to divide the iris, and with a hook to draw towards the outer lip of the wound till a sufficient gap is made. An aperture is thus effected by tearing and by the folding inwards of the flap. This may be denominated "incision with extension."

It not unfrequently happens that a dense layer of lymph or thickened capsule blocks up the pupil, and forms an impediment to "incision" with the knife—at least to the formation of a central aperture by it. Perhaps, too, without any capsule the iris is not healthy enough to retract when cut. With, however, the combination of another mode of operating, "excision"—the details of which will be subsequently given,—a pupil can be made towards the centre, and of better shape than by any other means. I have performed this compound operation many times. I divide the iris in the centre, as above described, with an iris knife; then seize the outer portion with an iris hook, draw it without the cornea, and cut off a piece. The chief nicety required in the operation is not to excise too large a bit.

[The mode of procedure described below, is that recommended in a case of partial closure of the pupil by lymph, the lens and its capsule transparent.]

Tearing with excision; irridesis.

Tearing out a strip of the iris, and then cutting it away, is the compound operation chiefly applicable here. The cornea should be opened at the margin, with a small iris knife or broad needle, a Tyrrell's hook introduced, the point carefully inserted into the opening of the pupil, which would necessarily be very small, and an endeavour made to tear out a piece of the iris. Sometimes a thin strip is brought away, which may or may not be snipped off, according to the length of the piece. Sometimes only a fissure is made, and therefore an inadequate aperture; the result depending very much on the condition of the iris, whether it be considerably altered in structure or not; if much damaged, the hook readily tears out. It may therefore be necessary at a future period to enlarge the fissure. The hook is more in contact with the capsule of the lens in this than in any other operation for artificial pupil; yet, if properly made and carefully used, it will not inflict any injury.

It matters not, as far as the performance of the operation is concerned, at which side of the eye the free portion of the pupil be situated, for the stem of the hook can be so bent as to allow the instrument to be used in any direction; but it is better, when the iris is unadherent in a direction not the best suited for a pupil, to disregard any advantage, so far as mere manipulation is concerned, and with the canula forceps make an aperture in the best situation.

The operation "iriddesis" is also applicable here, if not by one tying, by two, at a few lines apart, whereby a triangular aperture is made. It must be mentioned that the iris may be too rotten to be tied.

DIMINUTION OR CLOSURE OF THE PUPIL FROM PROLAPSE OF THE IRIS THROUGH THE CORNEA, OR ADHESION OF IT TO THE CORNEA IN CONSEQUENCE OF A WOUND, A PENETRATING ULCER, A SLOUGH, OR SUPPURATION OF THE CORNEA; THE CORNEA ITSELF MORE OR LESS OPAQUE IN THE CENTRE, THE LENS AND THE CAPSULE TRANSPARENT OR OPAQUE, OR THE LENS LOST.

Cutting or tearing through the adhesions; excision; incision; iriddesis; separation.

Under this head occur the greater number of cases requiring an artificial pupil; and of these the majority arise from ulceration of the cornea and prolapse of a part of the iris. But as, in a surgical point of view, it is the same whether the pupil be lost by prolapse or by mere adhesion of the iris to the cornea, I shall not practically recognise any difference.

When a part of the pupillary margin is prolapsed, the pupillary aperture being merely diminished, the corneal opacity limited, or, if extensive, not so dense as to obstruct light, the iris should be detached from the cornea. The cornea should be punctured, with the smallest iris knife, at the spot where the adhesion can be most readily reached, the blade directed between the iris and the cornea, and applied at once to the part to be severed. If the knife be carried too far and then withdrawn, the aqueous humour will escape, and the operation probably fail. I have torn away such connexion with a blunt hook. Should a large portion of the pupil be adherent, it is impossible to operate by these methods. In such cases I cut through the connexions with a blunt iris-scissors, or sometimes only some of them. Less frequently, I have used the scissors to the utmost, and torn through the remaining adhesions with a blunt hook.

"Excision" is often well adapted. I have made excellent pupils by it.

When the pupil is quite closed, the choice of the operation must depend chiefly on the extent of the corneal opacity, and the state of the lens.

When the lens is present, and supposed not to be opaque, the question is whether a portion of the iris should be torn away with the capsule forceps, as already described, or Tyrrell's operation adopted—that of incising the iris close to the cornea with the smallest iris knife, and with the blunt hook withdrawing a portion of it, and then cutting it off.

When the lens is absent, in consequence of its having escaped through a wound, or having been absorbed, "incision" is applicable, in combination, perhaps, with "excision."

When cataract is present, the operator must decide, according to the rules already laid down for parallel instances, whether he shall extract it at the time of making the false pupil or subsequently, or whether he shall perform the operation for the pupil, and afterwards treat the cataract by "solution."

When the pupil is closed from extensive adhesion of the iris, and only just the circumference of the cornea is transparent, "separation," or the tearing away of the iris from its natural attachment, is the only resource; for then it would be most imprudent to incise that part of the cornea for the execution of any of the other operations, lest opacity of it should ensue.

"Separation" therefore should be restricted to those cases in which none of the above-described operations can be executed, as it inflicts much injury on the eye, and according to my own experience, is less successful than any of the methods of making a false pupil that I have described. Besides this, it may be stated in general, that eyes requiring it are most unfavourable for an operation, as they are usually the wrecks of disease, and fall only just within the compass of operative surgery. The cornea, besides having dense opacities from slough or penetrating ulceration, is frequently staphylomatous; and adhesions of the iris to it diminish, or even destroy, the anterior chamber, whereby the operation is rendered very difficult.

To perform the operation of "separation" the cornea should be divided obliquely with the second-sized iris knife, opposite, or as nearly as possible opposite, the portion of the iris to be detached, and so far only from its margin that, when enough of the iris is torn away, it shall readily reach the incision, so as to allow of a part of it being withdrawn, and cut off or strangulated. If the incision be very far, too much will be separated. The next step is the important one of extracting the piece of iris. The canula forceps are very applicable, and I have used them with very perfect satisfaction. They are far preferable to the hook, because they more certainly retain the tissue of a diseased iris, and are less likely to injure the capsule of the lens, and are easier withdrawn. The hook that I have employed is the ordinary "lens hook" sometimes used in the operation for cataract.

Whichever be the instrument selected, it should be carried in front of the iris to the margin, where it is implanted and separation effected, and as the circumference of the iris is a little behind the sclerotica, the extremity must pass out of sight to seize it. Then the instrument must be kept close to the cornea while being withdrawn, lest the capsule of the lens be injured. When the hook is used the withdrawal through the cornea requires adroitness, and unless the aperture be ample, the iris will probably be shaken off; the size should, therefore, bear some relation to that of the hook, being, of course, always much larger.

There is greater uncertainty in the execution of separation than in any of the foregoing operations. Instead of a portion of iris of the required size separating, a mere strip of it may give way, or what is more common, the instrument may tear out without effecting any separation.

I must consider in this place that displacement of the pupil, and generally, too, that diminution of it, which may occur after the operation for "extraction," and which is nearly always produced by prolapse of the iris. When the aperture is very small, and the iris therefore tense, I have enlarged it by simple "incision" with the knife. When, although perhaps large enough, it has been too much displaced to be of full benefit—that is, by being concealed under the upper eyelid, "incision" with the knife is not generally applicable, but it may be done with the scissors. I divide the cornea to the required extent with the cataract knife, and with the scissors I make a snip in the centre of the free edge. I have had occasion to adopt "excision."

There are a few topics that may for convenience be introduced here as concluding general remarks.

The classification I have made has been solely for the sake of perspicuity and brevity, and I hope it has been made sufficiently clear, that the choice of any particular operation must be based on a careful examination and analysis of all the particulars of the case. The peculiarity in each instance must be the guide to the method most suitable.

An operator should not follow implicitly any rules that have been given, if he should deem it advantageous to depart from them. I profess to give only general principles. It is practically impossible to supply minute details for the guidance of a beginner in any class of operations.

The success of a case must often depend on the manner in which the corneal incision is made, whether sufficiently ample or not, and the selection of the best position for it. If it be too narrow it should at once be enlarged with the secondary knife, and if it be more convenient to make the incision at the line of junction of the cornea and the sclerotic, or even through the sclerotic half an inch behind the cornea, such spots should be chosen.

When the size of a pupil is a matter of consideration, it should be remembered that "excision" invariably gives the largest aperture. The smallest is mostly got when a piece is torn out of an adhering iris.

The bleeding which may ensue is sometimes a hindrance to the subsequent steps of the operation. If it be limited in quantity, slight pressure on the cornea may squeeze enough out to clear the objects. If the hemorrhage continue, the operation should be suspended, and pressure applied on the eye by cotton wool and a bandage. If blood be poured out into the chambers of the eye after an operation, supposing it to be confined there, and to have ceased to flow, there need not be any apprehension. I am not aware that it is attended with any disadvantage; it is soon absorbed, and does not require any special treatment.

If an artificial pupil should close, and the eye be not too much damaged by the inflammatory action that has closed it, a second attempt may be successful. I have known a third required before a result was obtained. It is possible that atropine may be beneficial after operating, by keeping the edges of the pupil more apart. It should always be used, except when a part of the iris has been left strangulated.

The after-treatment is the same as when the operation for "extraction" has been performed, the most important points being the quiet of the patient, perfect rest to the eye, cold lotions, and, if there be pain, opiates.—*Lancet, June* 1 *and Aug.* 31, 1867, *pp.* 659, 257.

90.—ON A METHOD OF OPERATING FOR DIVERGENT SQUINT.

By Dr. C. R. Agnew, Lecturer on Ophthalmic Diseases at the College of Physicians and Surgeons, New York.

The method is as follows :—The patient having been placed upon his back, and under the full influence of an anæsthetic, and the eye to be operated upon exposed by the wire speculum, an assistant draws the cornea as much as possible towards the outer canthus by catching the tissues over the tendon of the external rectus muscle in the blades of a pair of fixation forceps. The operator then makes a horizontal opening over the internal rectus muscle, midway between its borders, and extending from a point one line distant from the cornea down to the semilunar fold. This opening should be made by lifting a vertical fold of the conjunctiva and sub-conjunctival tissues with forceps, and cutting it with scissors in a horizontal direction. If care is exercised, the internal rectus muscle will be exposed without any difficulty, or the occurrence of much bleeding. The next step is to secure the entire tendon of the muscle which is to be brought forward. This is especially essential in those cases in which the divergent squint has been the result of the operation for convergent squint, for in such cases the tendon and theca, having been much haggled in the original tenotomy, fall back irregularly, and, being split more or less, form false insertions, which are zigzag or interrupted.

After the apparent insertion of the muscle has been brought into view, a strabismus hook, having an eye drilled in its free extremity, and armed with a waxed silk, is made to sweep beneath it, from below upwards, care being taken to keep the instrument in close contact with the sclerotic, and carried so far back as to include every straggling band of muscle or theca which is to be advanced.

The uplifted mass should then be tied close to its sclerotic implantation. The next step is to divide the external rectus muscle freely through a vertical wound in the conjunctiva, and thus complete the preliminary steps for the advancement of the internal rectus. The latter step is effected as follows:—The operator, holding the ligature firmly in one hand, should cut with scissors the insertion of the muscle, and gently break up any bands of connective tissue which may attach it to the sclerotic. As he does this, he should draw upon the ligature and sway it from side to side until it becomes evident that any adhesions which might obstruct the advancement of the muscle, have been overcome. He should now estimate the amount of adduction which may be necessary to cure the divergence. This he can do by catching with forceps the sclerotic edge of the cut tendon of the external rectus, and drawing the cornea towards the inner canthus, while he holds upon the stretch the muscle to be advanced. The retentive sutures are now to be placed. For this purpose two delicate, short, and sharply curved needles are to be armed with fine, well waxed silk, and adapted to a needle-holder.

Having measured the extent to which the eyeball must be adducted in order to correct the divergence, the sutures should be passed as deep in the muscle and theca as may be necessary. The muscle and theca should be drawn well out and kept upon the stretch, so that the sutures may be passed as deeply as possible behind the caruncle, to secure a firm hold, and to leave a somewhat longer mass between the perforations made by the sutures and the ligature upon the cut end of the muscle than the original divergence measured. The course of the sutures should be perpendicular to the plane of the muscle, one passing through near its upper margin, and the other near its lower. After the sutures have been placed in the muscle the end included in the ligature should be cut off, care being taken to leave enough to prevent their tearing out. The amount cut should nearly equal the degree of divergence to be corrected. allowance being made for shrinking which has followed the detachment of the muscle from the sclerotic. The next step is to carry the sutures beneath the conjunctiva above and below the cornea. It is better to place the upper suture first. This also requires the curved needle. The point aimed at in carrying the needle along the sclerotic, beneath the conjunctiva, should be about a line above the cornea and over the centre of the line of implantation of the superior rectus muscle, and there the suture should emerge. Before tying the upper, the lower suture should be brought out at a corresponding point over the inferior rectus insertion. While the operator is cautiously tying the sutures, his assistant should, catching hold of the insertion of the external rectus, carry the cornea towards the internal canthus as much as possible, and thus effect what may be considered the real intention of the operation, namely, to adduct the eye strongly, and place the end of the *shortened* internal rectus in coaptation with the sclerotic at or near the natural line of sclerotic implantation. The exercise of a little care will cause the muscle to spread out and be hidden behind the horizontal pillars of the wound, through which the retentive sutures have been carried, and thus insuring a consolidation of the wounded parts, obtain the aid of the subsequent cicatricial contraction of the soft parts intervening between the cornea and the caruncle in the ultimate result. Whether this method be adopted throughout or not, I am quite sure that the use of the strabismus hook, armed with a ligature, will be found of great advantage in getting control over the tendon to be advanced, and making the surgeon feel sure that he has secured the entire mass. I have employed the method upon two eyes which had been rendered divergent by operations for convergent squint performed by a wandering quack. In one of these eyes the divergence was more than five lines; in the other about four. In the first eye I induced a small amount of convergent squint, which was cured by applying the glass, which neutralized existing hypermetropia. In the second case I produced a very slight depree of convergence, not exceeding a line, which was also removed by the use of the proper glasses. And in both these cases the existence of hypermetropic asthenopia, would have rendered glasses necessary, aside from any convergence.

I have also satisfactorily performed the operation in two cases in which slight paresis of the internal rectus, the result of injury, had led to divergent squint.

In one case, in which the divergence was so great that one-fourth of the cornea was buried beneath the external canthus, the result was only partially successful; but I propose to repeat the operation after the cicatrix over the region of the internal rectus has so matured and softened as to render a repetition of the steps possible.

I have not seen any inflammation of an annoying character following the procedure, and my experience thus far justifies me in employing the method in preference to any other, as I believe that the risk to the eye is very much less than by any other, and the probability of success greater. I believe that the chance of success is greatly increased by dividing the external rectus of the fellow-eye, even though you propose to advance the internal rectus of one eye only.—*New York Medical Journal, July* 1867, *p.* 317.

91.—ON THE REMOVAL OF OPACITY OF THE CORNEA.

Some very curious facts relative to the action of sulphate of soda on the cornea, are pointed out in a memoir just read before the French Academy by M. de Luca. It occurred to him that as sulphate of soda retains the fibrine of blood in solution, it might have the effect of dissolving away opaque portions of the cornea. At first he tried the experiment with solution of the sulphate in distilled water. The fluid he dropped into his patient's eyes, and the result was found to a certain extent satisfactory. Believing that the solid sulphate would produce still better results, he employed it in the state of very fine powder ; dropping it into the eye of the patient. This method proved to be most effectual. In one or two cases where the opacity was so decided as to produce nearly total blindness, a considerable amount of vision was restored.—*Medical Press and Circular, Sept.* 11, 1867, *p.* 251.

92.—ON THE PATHOLOGY OF TINNITUS AURIUM.

By John Bishop, Esq., F.R.S.

Every practical surgeon is well acquainted with the numerous accidents to which the bones of the head and the membranes of the brain are liable, and is aware that some of these may and really do affect the mechanism in the labyrinth, more especially since we know that the membranous tissues, as well as those of the osseous framework of the parts, are all liable to become inflamed. Let us now ask ourselves what are the cause and seat of those sounds in the ear which arise spontaneously, and which not only trouble the invalid, but which cause so much embarrassment for the aurist to relieve. It cannot be true, as some have supposed, that this malady arises either from the spontaneous movements of the organs external to the labyrinth, or otherwise from the immobility of the membrana tympani, as Sir W. Wilde has imagined, since we know that by plugging the external meatus, so as to prevent the vibratory movements of the external air from reaching the tympanum, we merely hear the sounds produced by the rush of blood in its transit by the ear to the brain, and we can very clearly distinguish the sound of a tuning-fork in the stopped ear when applied in a state of vibration to the opposite side of the head, whilst the ear whose auricle has been left perfectly free receives no sound, thus proving that the vibratory movements are transmitted to the stopped ear by the bones of the head to the semicircular canals, as Weber has suggested. However, the history of the cases under consideration proves that they take their origin in a deeper seat than the external organs of hearing. The cases of tinnitus aurium which I have more recently examined have been caused by exposure of the head in warm or tropical climates, or they have occurred in persons who have suffered from violent blows on the head on being thrown from open carriages, &c. If the external organs of hearing were the seat of the malady, it might justify some of those experiments which have so often been tried and have so signally failed. If we include in the term all the sounds affecting the organ of hearing resulting in defective states in the external mechanism, as well as those within the labyrinth, the seat of this complaint must necessarily remain vague and undefined ; but if we restrict it to those sounds which are traceable to some existing defects in the nervous structure of the labyrinth, we shall then be enabled not only to form a more correct diagnosis of the malady, but shall be able to form our determination of the kind of treatment best calculated to procure relief, if not entire recovery.

The most practical and common-sense views for the treatment of these affections have been discussed in the work by Sir W. Wilde, who is very justly severe against the nostrums recommended by empirics. He puts no faith in the vaunted virtues of galvanism, which might have been predicted to be an agent unsuited to the pathological states of these nervous structures ; and with respect to the injection of liquids, vapour of ether, or any other agents into the tympanum through the Eustachian tube, as recommended by Kramer and practised by others, he declares that he has never seen the slightest benefit from this practice, either in cases under his own care, or under the treatment of others. According to Cheselden, the injection of water into the Eustachian tube was followed immediately after by deafness ; and although Saunders has described a case in which the hearing was improved as long as water was retained in the ear, this effect might have been produced by its supplying a medium for the transmission of sound : at any rate deafness returned as soon as the fluid was withdrawn. Indeed, it is difficult to discover, on any acoustic or therapeutic principles, that good can arise from injections into the Eustachian tube ; and experience teaches us that its practice is often productive of mischief. It is observed that the symptoms which accompany tinnitus aurium in severe cases are confused noises in the ears during the whole of the day, accompanied with pains in the head, and often complicated with some mental confusion. In others, the noises occur periodically, and most commonly at night, as soon as the head is laid on the pillow; when either a tinkling, or a roaring noise like the waves of the sea breaking on the shore, has been heard. These symptoms are often aggravated by extraneous noises, and by intolerance of loud sounds. In cases such as these, the sense of hearing is greatly impaired. The history of the extraneous or internal disorders which prevail either before or at the time when the symptoms commenced will furnish the best data for forming a correct diagnosis of the course and seat of the affection; but in doing this, great care and attention should be given, not only to the statements of the patient, but to the discrimination of his power of giving a correct account of his symptoms, and to the history of the causes by which they were assumed to be produced.

The maladies of the internal ear form one-third of the cases of deafness; and since these are seated in the labyrinth, a part inaccessible to manual operations, we see the fallacy of attempting to cure these affections by tampering with the external ear. The delicate and complex structure of the labyrinth requires that it should be protected by a strong, dense, and elastic covering, such as is found in the bony structure by which it is enveloped; but notwithstanding these natural means for security, they afford no absolute immunity from inflammatory attacks and extraneous injuries. With regard to the treatment of these intractable cases, it is obvious that when mental excitement prevails from irritation of the membranes of the brain, the exhibition of sedatives is necessarily indicated; and in the chronic state of inflammatory action of long standing, a seton in the neck, kept open during a lengthened period, has been productive of salutary effects, and it seems to be more beneficial than the usual treatment of applying blisters and counter-irritants behind the ears. The constitutional treatment must necessarily depend on the condition of the patient and the functional derangement concurrent in each case. It is owing to their variable states and conditions that much must be left to the discretion and judgment of the surgeon. It has been reported by Sir Gilbert Blane that two cases of tinnitus aurium were cured by means of belladonna. One was relieved after a short interval by its application to the ear externally; and the other was cured in three days by the administration of one-third of a grain of the same remedy three times a day. The symptoms were noises in the ears, like the rushing of wind. However, it it is obvious that these cases described by Sir Gilbert Blane could not have had their seat in the vestibule or in any portion of the labyrinth; nor could they have been of the aggravated form we have been considering, since much more energetic treatment, and much greater length of time and care, have failed to procure relief; indeed, the second case appears to be an instance of nothing more than what is sometimes produced by the mere presence of wax in the auricles, or the effects of cold on the external ear. But when we turn to the consideration of cases of tinnitus aurium, arising from irritable conditions of the membranes of the brain affecting the mechanism of audition in the labyrinth, or morbid affections of the bones which protect the nerves of the internal ear, we may easily arrive at the conclusion that it is only by appropriate constitutional and local treatment that we can expect any satisfactory results; and that tampering with the ear by means of galvanism, injection of fluids or vapours, must necessarily be injurious to the patient; and we have the melancholy example of the fatal effects of injections into the tympanum of powerful poisons in the case of the late Mr. Toynbee.

In conclusion, it is now pretty well known to the medical profession that the greater the difficulties, and the more intractable the maladies affecting the human system, the greater are the opportunities for empirical practice. No faith should be placed in the agents for assisting the functions of the ear—such as are called "organic vibrators," "atomised fluids," &c.; and the same remark will apply to another subject, in the term "visible speech," since the phenomena of sight and hearing are separated physically in such a manner as to render the sight of a sound, or the perception of light by the ears utterly impossible. These phrases may therefore serve to amuse, if not to mislead the ignorant, but they cannot bear the test of scientific research.—*Lancet, July* 27, 1867, *p.* 96.

93.—NEW MODE OF EXAMINING THE MEMBRANA TYMPANI.

By Dr. W. Spencer Watson.

[There have been introduced within the last few years several forms of ear speculum, both convenient and efficient for the purpose intended. Dr. Watson thinks that focal illumination by means of a biconvex lens has some advantages not possessed by other means of illuminating the membrana tympani. It must be used in the same way as ophthalmic surgeons employ it for the examination of the crystalline lens and the vitreous chamber of the eye.]

The objection to the use of a polished speculum is that the reflected image of the membrana tympani or the lining membrane of the meatus causes some amount of confusion to the observer. Such, at any rate, has occurred to myself in the very limited experience that I have had of its use, and I have consequently had the inside of a bivalve speculum (made for me by Mr. Matthews, of Portugal-street) blackened, and without any reflecting surface whatever.

To compensate for the loss of illuminating power which the absence of a reflecting surface necessarily brings with it, I use a biconvex lens of 3½ inches focal length, which I hold at such a distance from the meatus that a cone of light is directed on the surface of the membrane. This has the effect of strongly illuminating the membrane itself, and at the same time keeping the surrounding parts in comparative darkness. For this purpose the lamp or other artificial light should be four feet from the head of the patient, and behind the head of the observer.

It is to be observed that the cone of light should be of such a length that a diffused light should fall on the membrane, and the latter should be a little beyond the focus of the lens ; otherwise the illuminated part would be too limited in extent to be practicably available for the purpose. By moving the lens nearer or further away from the meatus, this object can easily be obtained. When the meatus is sufficiently large in calibre, the speculum can of course be dispensed with.

I have generally found that, when obtainable, direct sunlight gives the best view of the membrane. But of course, if this means of illumination is used, it will not be safe to use a lens at the same time, nor is it at all necessary. It is, I think, a great advantage, when direct sunlight is used, to have a blackened speculum, if a speculum be employed at all.—*Medical Times and Gazette, Aug.* 31, 1867, *p.* 239.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

94.—ON THE ADMINISTRATION OF NARCOTICS TO PREGNANT WOMEN.

By the late Dr. ADAMS, of Banchory.

[Amongst the late Dr. Adams's posthumous papers, his son, Dr. Leith Adams, of the 22nd Regiment, has found the following article; also a paper "On a Variety of Impetigo Contracted from Cattle," which the reader will find in the Edinburgh Medical Journal for November 1867, p. 422.]

I have long been convinced that the administration of opium in full doses to pregnant women is highly dangerous to the child, and wish to call attention to the subject as being a point of great practical importance which would not appear to be sufficiently attended to. It must have struck every person of experience how easily infants are narcotised by opium. It has been affirmed that a single drop administered to a very young child has proved fatal. Mothers giving suck have frequently told me of the striking effects which opium and other medicines, taken by themselves, have produced on their sucking infants. One may readily infer, as the relation between the mother and child during gestation is more intimate than during lactation, that any powerful substance affecting the nutrition of the mother must have a still more powerful effect on the child in the former case than in the latter; and experience proves this to be the fact. A good many years ago, a midwife, whilst describing to me the case of a female of my acquaintance whom she had lately delivered, said, that when called to the woman, as she found that labour pains were slow of coming on, she gave her patient "a good large dose of laudanum," which suspended the process for about six hours; afterwards the pains set in strong, and in a very short time the child was born. I asked, "Was it alive?" The woman *naively* replied, "She never was more disappointed in her life, for that it was a remarkably fine, plump child, but was born in a dying state, and in spite of all she could do it never could be awoke from the lethargic condition, and died soon afterwards." I cautioned the worthy obstetrix against using the same medicine in future. I think I am also confident that opium given in cases of threatened abortion almost invariably ends in the expulsion of a dead fœtus, and therefore I think it should never be given unless when all hopes of prolonging gestation are gone. From personal experience, then, and reasoning from analogy, I cannot but think that the administration of opiates to pregnant women ought to be proscribed. I should be glad to know what is the experience of my professional brethren in such cases, as my attention has been lately directed to several published cases of a nature similar to that here stated.—*Edinburgh Medical Journal, Nov.* 1867, *p.* 422.

[How far Dr. Adams may be right in explaining the mode of action of the opium we cannot say; this, however, we have often noticed, that opium will act like secale cornutum and cause almost a violent *uninterrupted* action of the muscular parietes of the womb, thus stopping or considerably diminishing the circulation in the womb, and consequently, if prolonged too far, causing the child to die of *asphyxia.*—Ed.]

95.—OBSERVATIONS ON THROMBOSIS AND EMBOLISM OF THE PULMONARY ARTERY AS A CAUSE OF DEATH DURING THE PUERPERAL STATE.

By Dr. W. S. Playfair, Assistant Obstetric Physician to King's College Hospital.

[Out of twenty-five cases of death due to pulmonary obstruction in the puerperal state, in twelve there were either well-marked symptoms of phlegmasia dolens, or the veins of the lower extremities contained coagula. In all these cases death occurred after the 18th day from delivery. In all those cases in which no clots were found in the peripheral veins, with a single exception, death occurred before the eighteenth day. This difference is curious, but it is capable of explanation.]

The first step towards the production of an embolus is admittedly the formation of a coagulum in some of the peripheral veins. But no sooner is the fibrin deposited than it begins to undergo certain changes, which have been carefully studied by Virchow and other pathologists, the primary object of which is evidently to produce absorption of the coagulum, so as to restore the circulation in the occluded vessel. It is unnecessary to describe here what these changes are. They consist chiefly in the retrograde metamorphosis of the fibrin, which is generally either an amylaceous or fatty degeneration. The result is that the coagulum becomes softened; and then some accidental cause gives rise to the detachment of a portion, which, being carried to the right side of the heart and thence to the pulmonary artery, at once produces the phenomena of embolism. It can be readily understood, therefore, why the fatal result should occur after a considerable lapse of time, and when convalescence seems to have been fairly established. For, first, we have the local clotting, due very probably to a similar blood dyscrasia as that which produces spontaneous thrombosis in the pulmonary artery; and it is not till the clot so formed has had time to change and soften that portions of it become detached and form the embolus which proves fatal. When spontaneous thrombosis occurs, on the other hand, it must arise from the state of the blood rendering it peculiarly apt to coagulate; and this is more likely to be met with at a much nearer period to delivery. The pulmonary obstruction may then, indeed, be considered as taking the place of, and occurring about the same time as, the peripheral coagulation in undoubted cases of embolism.

It is interesting to observe how the condition of the blood in the puerperal state increases the tendency to thrombosis, and in this we have a sufficient explanation of the frequency of its occurrence after delivery. Indeed there is probably no other condition of the body in which so many circumstances concur to favour coagulation. For not only is there an actual excess of fibrin as the result of pregnancy, but a vast quantity of material destined for excretion is circulating in the blood for some time after delivery, the product of the involution which is going on to reduce the uterus to its usual size. And, in addition to this morbid constitution of the blood, an examination of the cases will show that when spontaneous thrombosis has occurred, the patient has, in a large majority of instances, been in a very feeble and exhausted state. Thus, out of the fourteen cases in which no clots were found in the veins after death, in no less than eight was there either post-partum hemorrhage, or the patient was expressly described as being extremely weak and anæmic. Now, taken by itself, the state of the blood thus produced would, independently of all other circumstances, greatly increase the tendency of the fibrin to become deposited. Dr. Richardson's opinion is very clear on this point. "There is," he observes, "a condition which has been long known to favour coagulation and fibrinous deposition. I mean loss of blood, and syncope or exhaustion during impoverished states of the body. The explanation here is, that these states are attended, as a general rule, by an excess of fibrin in the blood, as well as by an excess of water. The fibrin solvent is thus widely distributed, the density of the blood is reduced, the fibrin superabounds, and stasis only is required to give all the circumstances favourable to deposition." When, therefore, we have the alteration so produced, added to the already vitiated state of the blood due to puerpery, we cannot but regard the occurrence of thrombosis as a very probable circumstance.

It would be a question of great importance to decide if there is a sufficient difference in the character of the clots found in the pulmonary arteries to enable us to decide by inspection whether they are due to spontaneous thrombosis or embolism. Dr. Ball seems inclined to answer in the affirmative. He believes, if I understand him aright, that in a clot depending on embolism the embolus may be observed of a similar texture to the clots in the peripheral veins from which it was originally derived, surrounded by more recent fibrin, which can be readily distinguished from it; while, on the other hand, clots of spontaneous origin are identical in appearance with thrombosis of the peripheral veins, being dense, firm, white in colour, homogeneous in structure, and having a smooth, rounded head pointing in the direction of the heart. This form and direction he explains on the hypothesis that spontaneously formed clots always originate in the minute ramifications of the pulmonary arteries, gradually creeping backwards towards the larger branches. For this assumption, however, I do not see sufficient grounds. Dr. Ball considers it difficult to believe that the blood can coagulate in the primary divisions of the pulmonary arteries, because the propulsive movements of the heart would disturb it, and effectually prevent the separation of the fibrin. Dr. Humphry, however, has shown that the anatomical arrangement of the artery is such as to favour stasis and coagulation of the blood in the main trunks at the root of the lungs. He observes that "In this situation the artery breaks up at once into a number of branches, which radiate from it, at different angles, to the several parts of the lungs. Consequently a large extent of surface is presented to the blood, and there are numerous angular projections into the current; both which conditions are calculated to induce the coagulation of the fibrin." And if, as I have pointed out, it be true that spontaneous thrombosis generally occurs in exhausted and enfeebled patients, subject to syncope, and in whom there is a very weak action of the heart, it seem to be by no means unlikely that the deposition may primarily take place in the larger branches, where it is found after death. However, I think that we are scarcely yet in a position to speak dogmatically on this point. When more attention has been paid to the characters of the clots in various cases, and when the results of a larger number of post-mortem examinations have been recorded, it is by no means improbable that much light may be thrown on the origin of the obstruction.

A question of great interest arises as to whether recovery does not in some cases take place after the pulmonary artery has been occupied by a coagulum. This is a point of much importance, which has as yet scarcely received any attention. We are every now and then meeting with cases after delivery in which there are formidable symptoms of syncope, attended by embarrassed respiration and all the phenomena usually ascribed to embolism, in which, nevertheless, a more or less perfect recovery eventually takes place. Such cases have been usually ascribed by authors who have written on the subject to what they call "idiopathic syncope," a term invented to account for phenomena otherwise inexplicable. The symptoms, however, are so precisely those of pulmonary obstruction that it seems far from unlikely they may really be due to that cause. Mr. Paget has published a case of pulmonary thrombosis in a male patient, from which he draws the inference that "in certain circumstances a great part of the pulmonary circulation may be arrested in the course of a week (or a few days more or less), without immediate danger to life, or any striking indication of what has happened." Virchow also found clots in the pulmonary artery in several cases in which there had been no symptoms during life to give rise to a suspicion of their existence.

The history of many of the fatal cases render it quite certain that the deposition of the fibrin had existed for a considerable time without causing much distress, until some unusual exertion induced a call for blood which could not be supplied through the obstructed vessels, and hence the sudden syncope which carried off the patient. Is there anything improbable in the supposition that some of the cases of so-called "idiopathic syncope" may really have been of the same kind, with this exception, that the patient rallied and survived, and that the obstruction in the artery has been eventually removed?

Clots in the peripheral veins are certainly frequently absorbed, and the circulation in the occluded vessels becomes as perfect as ever. So strong is the tendency to this that Humphry observes with regard to it:—"It appears that the blood is almost sure to revert to its natural channel in process of time, unless the vein be completely destroyed." If this, then, be so frequent an occurrence in the venous system generally, surely it is more than probable that it may now and then occur in the pulmonary artery also. The following cases, which have come under my own observation, seem to me to be explicable on no other hypothesis.

Case.—Post-partum hemorrhage; symptoms of syncope on the eighth day; recovery.—On the 27th of April, 1864, I attended a young lady, 23 years of age, in her first confinement. She was a healthy person, and had a perfectly natural labour. It was followed, however, by hemorrhage, but not to any excessive amount; and this was soon checked by pressure on the uterus, the administration of ergot, and cold applications. On the sixth day the lochia became very fetid, but were not diminished in quantity. In all other respects she seemed very well. She was, indeed, weak from loss of blood ; but her pulse was under 100, the skin was cool and moist, there was no tenderness over the abdomen, and there was an abundant secretion of milk. I ordered her injections of tepid water and Condy's fluid, to remove the fetor of the discharges. On the next day she had one or two fits of hysterical crying, and felt low and depressed. The lochia was still intensely fetid, but she was no worse in any other way. On the following morning (the eighth from her delivery) she awoke feeling better, and ate a hearty breakfast. About half an hour afterwards the nurse, going to her bedside, found her in a state of collapse, gasping for breath, and unable to speak. She immediately gave her some brandy, but with little effect. I was sent for at once, but in the meantime, her condition being so alarming, they also sent for the nearest medical man, Mr. Willington, of Sumner-place, who administered brandy, ammonia, chloric ether, &c., freely. I did not reach her until an hour and a half after her first attack, when I found her somewhat rallied. She was then ghastly pale, with an anxious, pinched look about the face. The pulse was 110, soft, and compressible. The respirations were only slightly increased in frequency, and no abnormal sounds were audible about the heart and great vessels. She was ordered a mixture containing sesquicarbonate of ammonia every hour, and a dessertspoonful of brandy every second hour. The most absolute rest was enjoined, and the nurse was directed not even to allow her to raise her head off the pillow. She felt "faintish" every now and then for several days, but eventually made a perfect recovery, and has since had another child, after a natural and easy labour.

The second case is perhaps still more to the point.

Case.—Severe post-partum hemorrhage; intense syncope a week after delivery; frequent recurrence of the attacks; recovery.—This case occurred in a patient of Dr. Arthur Farre's, with whom I attended. A young lady, of feeble and delicate health, was confined of her third child on the 19th of October, 1866. She had an easy labour, which was followed by very profuse hemorrhage. All the ordinary means—pressure, ice, the cold douche, ergot, &c.—failed to check the flow, which was eventually arrested by injecting the cavity of the uterus with a solution of perchloride of iron. She was much exhausted, her countenance was blanched, and she had sighing respiration. Perfect rest was enjoined, and for a week she seemed to be doing well, taking strong essence of beef, and six drachms of brandy every second hour.

During the whole of the seventh day after her confinement she felt unusually weak, complained of faintness, and said that her head felt dizzy whenever she moved it. About six o'clock the same evening she suddenly called out that she was dying, and fell into a state of syncope. She rallied on the administration of stimulants, and when I saw her, about an hour after the attack, she was still highly excited, and said she felt as if she was going to die; but the respiration was not then rapid; the pulse was feeble and small, about 130; the hands and feet were numb and cold. The most absolute rest was enjoined, and stimulants and restoratives were administered freely. For several days she remained in a very critical condition, the slightest exertion bringing back the tendency to faint. On one occasion I detected a faint blowing murmur at the base of the heart, but it was not permanent. The auscultatory phenomena were in other respects perfectly normal.

For nearly two months she continued in the same feeble state. As long as she remained in the recumbent position she felt pretty comfortable, but any attempt at sitting up in bed, or any unusual exertion, immediately brought on embarrassed respiration. It was also found necessary to continue the same amount of stimulants she had been taking from the first, as any diminution in the quantity at once brought on a tendency to syncope. She is, as I write, getting slowly and gradually better, but is unable to sit upright for more than a few minutes at a time.

The next case I did not see during the attack, but the circumstances were related to me by the patient herself, a person of great intelligence, and I took every means in my power to satisfy myself as to the details of her illness. I have therefore no hesitation in recording it, and believe her account of the case to be substantially correct.

Case.—Symptoms of pulmonary obstruction eleven days after labour; phlegmasia dolens; recovery.—On August 20th, 1866, I was consulted by a lady 44 years of age, the mother of twelve children. She complained of general weakness and debility, and she had phlegmasia dolens of the left lower extremity, which was hard and swollen up to middle of thigh. She informed that she had had an easy labour on the 6th of July. She made a good recovery, and on the eleventh night went to bed feeling very well. There was no swelling or discomfort of any kind about the lower extremities at this time. About half-past three a.m. she was sitting up in bed, when she was suddenly attacked with an indescribable sense of oppression in the chest, and fell back in a semi-unconscious state, gasping for breath. Her husband, greatly alarmed, went for the nearest medical man, who administered stimulants freely. She remained in this condition for about three hours, when she commenced to rally. The secretion of milk, previously abundant, was suspended. Two days *after* the attack, symptoms of phlegmasia dolens came on ; the left leg swelled, and has since remained in the same condition as it was when I saw saw her. She has since been very feeble and out of health, but there has been no recurrence of syncope.

Now in all these three cases the symptoms and history are exactly those of pulmonary obstruction. Had the patients died, and no post-mortem examination been allowed, I think no one would have hesitated a moment as to the diagnosis. They also illustrate well the points in the history of the disease I have endeavoured to explain. In two out of the three there was post-partum hemorrhage, and a tendency, therefore, to coagulation from the altered condition of the blood. In all the three the attack occurred before the twelfth day after delivery—just the time, as I have previously observed, at which spontaneous thrombosis most generally takes place. The last case was certainly complicated with phlegmasia dolens ; but this did not show itself until two days after the chest-symptoms. On this point there can be no doubt, as the patient was perfectly sure that, at the time of her attack, no swelling of the leg existed. I believe, therefore, that this is an instance of pulmonary and peripheral thrombosis occurring simultaneously, from some condition of the blood tending to coagulation. I have no doubt that many practitioners have met with similar cases, which seemed at the time inexplicable ; and it is in all probability simply because the attention of the profession has not been directed to the subject that more are not recorded.

Dr. Arthur Farre agreed with me as to the probable cause of the attack in his case, and informed me that he had seen more than one case in his practice which he now believed to have been caused by pulmonary obstruction, and in which the patients eventually made a good recovery.

If I have rightly interpreted these cases, they teach us the lesson that we should not entirely despair when we meet with a case presenting the symptoms of embolism. Unfortunately, the question of treatment is one on which but little can be said. By far the most important indication is, doubtless, to enforce the most rigid rest, scarcely even permitting the patient to move for a long time after the occurrence of the suspicious symptoms, so as to enable sufficient blood to pass through the obstructed vessels to carry on the animal functions until a free channel for the circulation is in the course of time restored. The history of all the fatal cases clearly shows how dangerous is any action which calls for an increased supply of blood ; for the fatal attack has almost invariably been the direct result of some unusual exertion. In addition to this, a free administration of stimulants, and a liberal supply of strong animal soups, will be indicated, to support the strength as much as possible. It is to be feared that no medicinal agents are likely to be of any service, except in the way of stimulants and restoratives.

No less than twelve out of the twenty-five cases occurred in primiparæ; while of the remainder there are several in which nothing is said as to whether the patient had previously borne children or not. Whether this is a mere accidental circumstance, or whether there is actually a greater tendency to deposition of fibrin in first than in subsequent labours, I am unable to say. It is sometimes said, on the other hand, that phlegmasia dolens is more common in multiparæ. Probably a much larger mass of statistics is required before we can come to a conclusion on this point, but as far as our present cases go the number of primiparæ greatly predominates.

Finally, I think the facts before us justify the following conclusions:—

1. Obstruction of the pulmonary artery after delivery may depend either on embolism or spontaneous thrombosis.

2. The former usually occurs at a much later period after delivery than the latter; and spontaneous thrombosis probably corresponds with, and is due to some cause similar in its nature to, that which produces the obstruction of the peripheral veins in true cases of embolism.

3. Both thrombosis and embolism are much more common in patients who are anæmic and weak, either from hemorrhage or other cause.

4. It is probable that obstruction of the pulmonary artery sometimes occurs without proving fatal.

5. The main element in the treatment of such cases is the most rigid rest, and a nourishing supporting regimen.

6. As far as present statistics go, thrombosis and embolism seem more common in primiparæ than in multiparæ.—*Lancet, July* 27 *and Aug.* 10, 1867, *pp.* 94, 153.

96.—ON SAFE DELIVERY FROM THE PAINS OF LABOUR.

[Mr. Isaac Brown, in his work on this subject, recommends]

For the purpose of procuring "painless parturition with full mental consciousness," one part of rectified eau-de-Cologne with two of chloroform. The result is, he says, a clear, colourless, and highly agreeable liquid, having a specific gravity of 1·152. —*British Medical Journal, Sep.* 7, 1867, *p.* 208.

97.—ON THE USE OF THE FORCEPS IN LABOUR.

By ARTHUR B. STEELE, Esq., Lecturer on Midwifery, Liverpool Royal Infirmary School of Medicine.

[The opinions given and the rules laid down by different writers on the use of the forceps are very various. Referring particularly to the following sentence from Dr. Churchill's writings, "in no case is the forceps to be applied until we are perfectly satisfied that the obstacle cannot be overcome by the natural powers, with safety to the mother and child," the writer observes :]

Those who fully appreciate the evil results of protracted labour, will hold that its fulfilment implies interference at a very much earlier period than would be deemed proper by others, who maintain that the natural course of labour should not be interfered with, until certain definite symptoms of approaching danger have appeared. What I desire chiefly to enforce is, that the use of the forceps should not be deferred until bad symptoms have appeared, but that they should as a general rule be applied at once, whenever the second stage of labour ceases to be actively progressive; excepting of course when contraction or distortion of the pelvis absolutely necessitates turning or craniotomy, or when the os uteri is only partially dilated, and at the same time undilatable.

I hold that it is not necessary to wait for six, four, or even one hour, so long as we are satisfied that the labour, if left to nature, will be tedious and protracted; a point which, I admit, may require much judgment and experience to determine. I altogether discard the rule that before the forceps are applied the ear should be felt, neither is it always necessary to wait until the os uteri is fully dilated; for with ordinary care the blades may be passed beyond the os uteri without inflicting the least injury, and with properly constructed forceps the os uteri is not stretched until the head itself is brought down to assist in the dilatation in the natural way.

When the fœtus is known to be dead, although under certain circumstances craniotomy, as usually recommended, may be more suitable, yet, as a general rule, I prefer to deliver a dead child by the forceps, for two reasons; first, because, as we have seen, perforation is fatal in one case in five, while forceps mortality amounts to one only in thirty-three; and, secondly, assuming even that craniotomy is not more dangerous than the forceps, it is objectionable on other grounds as being, in appearance at least, a repulsive and somewhat barbarous proceeding, even though the child be dead. It must never be forgotten that, although we often have clear and positive evidence that the child is living, we cannot so surely determine that it is dead, although the usual signs of its vitality are absent.

I reject, as opposed both to theory and to experience, the notion that the forceps ought never to be used as compressors. I believe, on the contrary, that when properly applied, and skilfully manipulated, they conduce materially to the moulding of the head in its passage through a tight pelvis; and I have more than once proved in practice that a child may be delivered alive and unharmed, when a considerable amount of compressing force has been required. I advocate the early application of the forceps, because the earlier we apply them the better will be the position in which we grasp the head, and because we thus anticipate the disadvantages which ensue when the head is left to press for a long time on the soft parts of the mother, causing congestion and effusion, and so leaving less room for operating at a more advanced stage, besides rendering the vagina more liable to injury.

I recommend, as the best instrument for all cases, the long curved forceps of Simpson or Roberton; and on this point I speak with much confidence, from considerable practical experience, having found them more easy of application, and much more satisfactory in their effects, than the short forceps. I have for several years abandoned the use of any other except Roberton's forceps, and I feel quite sure that any practitioner, who will give them a trial, will arrive at the same conclusion. The chief points to be borne in mind in operating, besides the general directions of authors, are as follows: The operator should have constantly in his mind's eye, the curved line described by the head in its passage thongh the parturient canal, and direct his extractive force in accordance therewith; it will almost invariably be found that the rotation of the head as it passes through the pelvis, will be effected by nature, without assistance from the instruments, thus rendering any rotatory motion with the forceps unnecessary. The extraction should never be unduly hurried; nay, often, when the head is at the outlet it is necessary even to delay it, and thus save the perineum. This is a use of the forceps which I think has not been sufficiently dwelt upon, and which can only be satisfactorily accomplished by the long curved forceps.

I repudiate the dogma, that force is never to be used in artificial delivery; in employing it judiciously we imitate nature herself, and it is only when abused that it becomes dangerous. I have over and over again verified in practice that, with proper precaution, and with appropriate manipulation, delivery may be accomplished with safety to both mother and child, even when considerable force has been necessary to effect the extraction of the head.

I regard the use of the forceps as more appropriate in those cases in which many obstetricians recommend the ergot of rye, or other oxytoxics, for the following reasons: it gives the accoucheur a more certain, regular, and complete power over the course of labour, especially during the exit of the head, enabling him either to hasten or retard its progress as circumstances may require.

Their use does not involve certain risks which are admitted to attach to the action of ergot, even by those who advocate its employment in tedious labour. Dr. Johnson, for instance, says, "That when ergot is given it brings matters to a crisis, for if the child be not delivered within a certain time it will in all probability be destroyed." Drs. Hardy and McClintock state they "have on many occasions observed the ergot of rye to exercise a very depressing influence upon the maternal circulation." More recently Dr. McClintock has advocated the view, taught by Dr. Robert Lee when I was a student, that danger to the child arises from the incessant uterine action, and the consequent pressure, which the ergot is so liable to occasion. Dr. Churchill attributes to its use "retention of the placenta, from irregular uterine action, after the birth of the child." (*Theory and Practice of Midwifery*, 1866, p. 264.)

In my own practice I rarely use it in actual labour, preferring it as a means of completing abortion, when all hope of saving the ovum is abandoned, or of producing contraction in *post partum* hemorrhage. It is my firm conviction that the more the forceps come into favour, the more rarely will ergot of rye be used, and that ultimately it will be altogether set aside as a substitute for what I cannot but regard as a safer and more efficient means of affording assistance in tedious labour.—*British Medical Journal, May* 11, 1867, *p*. 532.

98.—ON THE POWERS OF THE FORCEPS.

By Dr. Robert Barnes, Physician to and Lecturer on Midwifery at St. Thomas's Hospital, &c.

The Powers of the Forceps—the Force by which it holds the Head—the Compressibility of the Child's Head.—Three distinct powers or forces can be developed in the forceps. First, by simply grasping the head and drawing upon the handles, it is a *tractor*, supplementing a *vis à fronte* for the defective *vis à tergo.* Secondly, the forceps consisting of two blades having a common fulcrum at the joint or lock, we can by a certain manipulation use it as a *double lever*. Thirdly, if the blades and handles are long enough and strong enough, and otherwise duly shaped, the forceps becomes a *compressive power* capable of diminishing certain diameters of the child's head, so as to overcome minor degrees of disproportion.

Now, all these powers may be brought into use, and all may be in great measure lost, according to our choice of a good or a bad model. Thus, if we rest satisfied with the short forceps of Denman, we shall only have a feeble tractor, a feeble lever, and an instrument having almost absolutely no compressive force. It is obvious that such a forceps can have but a restricted application. It can only serve to deliver the child when the head is in the pelvis, when very little tractile power is required. Ask yourselves what this means. What are the consequences in practice? Simply this: you are driven in a multitude of cases to perforate, to destroy the child. Such an alternative may well make us reflect whether we cannot extend the powers and the application of the forceps. By simply lengthening the blades and shanks and giving the blades an additional curve adapted to the curved sacrum, we can reach the head detained at the brim of the pelvis. By moderately lengthening the handles and making the instrument stronger, we increase the leverage and tractile power, and we gain a moderate compressive power. Thus we bring within the saving help of the forceps a further number of children that must otherwise be given up to the perforator, or run the risk of turning. You ask, Why hesitate to endow the forceps with this great privilege? Why has the feeble forceps of Denman so long held its sway in this country? The reason is that there are limits beyond which we cannot push the saving powers of the forceps. If we pass beyond these limits, we run into danger of injuring the mother and of losing the child. Now, the great contest in all matters of strife is about boundary lines; and it is concerning these limits that authorities have differed. Some men are afraid of giving power, lest it should be abused. They are so terrified at the possible mischief which great power may work, that they would rather abandon the good which great power is equally capable of working. They tremble lest we should be unable to acquire the skill and the discretion necessary to direct the greater power. Such men virtually say, You shall not apply the forceps where the head has not descended into the pelvic cavity—an arbitrary limit dictated by fear, and fixed by ignorance, that the forceps is just as capable of safely delivering a child whose head is arrested at the brim. For here, as is continually the case in medicine, experience arbitrarily limited excludes progress in knowledge and bars improvement in practice. For example, how can a man acquire a just knowledge of the power of the forceps to deliver a head delayed by slight disproportion at the brim, if he always delivers under this difficulty by perforating? Clearly, he bars himself from acquiring that knowledge; and, giving up his intelligence to the delusive dictates of his wilfully limited experience, he refuses even to accept the evidence of those whose experience is greater, because it is directed by a freer spirit of research, by greater confidence in the resources of art.

Let us, then, go back to the study of the powers of the forceps, unshackled by any preconceived opinions as to what the instrument can do or can be permitted to do. First, as to its *tractile* powers. In order to draw, the instrument must take hold. How does it take hold? You may at first sight suppose that this is accomplished by grasping the handles. But in the case of the ordinary forceps, especially the short-handled forceps, there is little or no compressive power, so that the hold cannot be due to the handles. The hold is really due to the curvature of the blades, which fit more or less accurately upon the globular head, and the compression of the bows of the blades against the soft parts of the mother, supported by the bony ring of the pelvis. This may be made clear by a simple experiment. Take an india-rubber ball, slightly larger in diameter than a solid ring; place the ball upon the ring. Then seize the ball through the ring by the forceps. The blades will be opened out by the ball. Then drawing upon the handles, even without squeezing them together, you will see the blades pressed firmly upon the ball by gradual wedging, as the greatest diameter or equator of the ball comes down into the ring. Just so is it with the child's head and the pelvic brim and canal. The blades are held in close apposition to the head by the soft parts and the pelvis of the mother. In many cases this outward pressure upon the bows of the blades is enough to serve for traction. It is not necessary to tie the handles of the forceps. You may even do without handles altogether. Thus, one of the earliest attempts, stimulated by the desire to realise the concealed discovery of the Chamberlens—that of Palfyn—consisted in applying two opposed levers, which did not cross, and therefore could not exert any compressive force. Assalini's forceps was constructed on this principle. It is essentially a tractor, with slight leverage power. Professor Lazarewitch, of Charkoff, brought to the Obstetrical Exhibition a beautiful forceps constructed on Assalini's principle. This instrument I applied in two cases. It held admirably; but all its holding power is due to the pressure exerted by the mother's parts upon the blades.

Mattéi, of Paris, has made another instrument, whose blades do not cross, whose shanks parallel are set in a cross bar of wood to serve for traction; and quite recently Dr. Inglis has proposed a forceps in which the handles are done away with altogether, there being nothing but a short curve of the shank, representing the shoulders on the handles of Simpson's forceps, to serve for traction. I think this sacrifice of all compressive and leverage power, reducing the instrument to a weak tractor, is a retrograde movement. But it proves the proposition that the hold upon the child's head is the result of the adaptation of the curved blades and the outward wedge-pressure of the mother's parts upon the bows of the blades. Now, the strength of the hold depends mainly upon the degree of curvature of the blades and the width of the fenestræ. If the curve is one of large radius, so that the two blades, when in opposition, approach parallelism, and especially if the fenestræ be narrow, the hold will be feeble, and moderate traction will cause the forceps to slip, and this in spite of any compression you can exert upon short handles. But increase the curve so that the blades in opposition form nearly a circle, and the instrument will not slip. This increased head-curve is one feature of the French or Continental forceps. The hold is further strengthened by making the points approach nearer together. In the English patterns the points are generally distant from each other an inch or more. In the foreign forceps the distance is often much less than an inch. There is some danger from this proximity of pinching or abrading the skin of the face. So much for the grip and traction.

Let us now study the *compressive power*. This is inconsiderable in almost all the English forceps, but is an important feature in most of the foreign long forceps. The essential condition for compression is, indeed, present in English and foreign. This consists in the crossing of the blades, and in the greatest divergence of the blades, when the handles are brought together, being less than the greatest transverse diameter of the child's head. This diameter is normally from 3¾ to 4 in.; the greatest divergence of the blades is rarely more than 3 in. Therefore, when the blades are sitting loosely on the head, the handles diverge. Practically, the head is rarely grasped exactly in its transverse diameter, but generally in one more or less oblique—something between the transverse and the longitudinal diameter. This, of course, is even longer than the transverse. Now, if we are to exert any direct compression upon the head, we can only do it by squeezing the handles together. For this purpose, the handles must be long and strong on one side of the lock, and the blades must be strong, but not much longer, on the other side of the lock, than are the handles. It would be useless to provide this compressing power if the head were not compressible. That the head is compressible—that is, that we may diminish some of its diameters by lengthening others—is easily proved.

Firstly. It is known that in normal pelves the head in passing, if the labour be protracted, undergoes elongation; from round it becomes conical; the greatest transverse diameter—the interparietal—becomes merged in the lesser or interauricular, whilst the longitudinal diameters are correspondingly increased.

These changes I have demonstrated by actual measurements and outlines.

Thus, just as the pressure of the soft parts and the pelvis is a main agent in fixing the forceps upon the head, so it is in moulding the head to allow of its passing. Indeed, I think this pressure almost entirely accounts for the alteration of form the head undergoes when the English forceps is applied. I can show outlines of heads as strongly altered under the natural forces of labour as they often are under forceps delivery.

Secondly.—Numerous experiments have been made with strong forceps upon dead children to determine this point. Baudelocque found that he could lessen the transverse diameter by a quarter to a third of an inch. Siebold gained half an inch. Osiander and Velpeau claim quite as much. More conclusive are the observations of M. Joulin and of M. Chassagny. These gentlemen, in experiments designed to demonstrate the utility of continuous compression and traction by powerful forceps upon the head in difficult labour, have completely proved that a degree of moulding may be effected much beyond that commonly observed. This moulding consists in the elongation of the head, the elongation being gained by the lessening of the equatorial diameter. The process resembles that of reducing wire by drawing it through holes in an iron plate.

Now, another question arises: the head is indeed compressible, but to what extent is it compressible without sacrificing the child's life? For if the maximum of plasticity compatible with life is represented by that degree which is common in severe first labours, then we ought to give the mother all the ease in our power by lessening the diameter of the child's head by perforating. It is very difficult to fix this limit with accuracy. Baudelocque thought compression to the extent of a quarter or a third of an inch was compatible with the safety of the child. The important fact is, that in many cases the child survives, although its head has undergone very great compression and moulding.

The following conditions influence the result:—The degree of development of the head as to size and ossification; and the mode in which the compressing force is applied. If this force be applied *gradually* and *continuously*, a much greater extent of moulding with less injury to the child may be obtained than what Baudelocque thought possible.

At one time it was the practice—more probably with the view of securing the hold than of compressing the head—to tie the handles together; and even now that tying is generally abandoned and condemned, the old custom asserts itself in the preservation of the grooves near the extremities of the handles made to receive the ligature. The objection to tying is this—the continuous compression is opposed to the course of nature, which intermits the expulsive act, giving periods of rest during which it is presumed that the brain may better adapt itself, and its circulation be maintained. Hence the law that we ought in forceps labours, and, generally, in all operative labours, to imitate this intermitting action, by interposing intervals of rest, endeavouring so to time our efforts as to be simultaneous with, and in aid of, the natural expulsive efforts. The argument is good both in logic and in physiology. It is not wise to disregard it. But experience proves that there are cases where the moulding of the head can be accomplished more quickly, and without endangering the child, by continuous pressure. Some practitioners, therefore, have recurred to the old practice. Dr. Gayton has adapted a clip to the handles of the forceps, which answers much better than the ligature. Whatever the mechanism resorted to, it is essential that it admit of being instantaneously removed, in order to allow the blades to be taken off. Delore, who has made many dynamometric observations, concludes that pressure exerted either by the forceps or by the genital organs may be harmless to the head if spread over a large surface. It is limited and angular pressure that is dangerous. He has also shown that *the greater the traction the greater is the pressure.* The pressure is equal to about half the traction. Thus, if you exert a traction force of fifty pounds, the pressure upon the head is about twenty-five pounds.

To economise traction, then, is to economise pressure. How do we economise traction?

There are three principal rules.

First.—Take sufficient time to allow the head to mould.

Secondly.—Take care to draw in the axis of the brim—that is, traction must be perpendicular to the plane of the brim. If this is neglected, additional force is required, increasing with every degree of angular difference.

Thirdly.—To use slight movements of laterality or oscillation.

This uncertainty and inconstancy in the degree to which compression may be carried with safety to the child is a justification for tentative or experimental efforts with the forceps. It is the reason why, in doubtful cases, where the disproportion in size between pelvis and head is not very decided, we are called upon to make a reasonable trial of the forceps before resorting to craniotomy. It appears to me quite certain that in this country we are yet far from having utilised the powers of the forceps to the highest legitimate extent. I might go further, and say that during Denman's time and until quite recently we had actually lost ground in this respect, and had reverted to the use of instruments scarcely better than the original rude forceps of the Chamberlens. More than one hundred years ago Smellie contrived and used the long forceps. Perfect used it, and it seems that in his time the long forceps was better known than it was at the beginning of the present century.

Application of the Long or Double-Curved Forceps.—The application of this instrument is governed by a different law from that which governs the use of the short forceps. The short forceps, according to the recognised rule, must be applied with the blades quite or *nearly* over the transverse diameter of the head. The head determines the manner of applying it. But with the long forceps it is the pelvis that rules the application. The position of the head may be practically disregarded. The pelvic curve of the blades indicates that these must be adapted to the curve of the sacrum in order to reach the brim. They must therefore be passed as nearly as may be in the transverse diameter of the pelvis. One blade will be in each ilium, and the head, whatever its position in relation to the pelvic diameter, will be grasped between them. The universal force of this rule much simplifies and facilitates the use of the instrument. Not only does it apply to the position of the head in relation to the pelvic diameters, but also to all stages of progress of the head from that where it lies above the brim down to its arrest at the outlet.

It has been contended that the short forceps should be preferred in cases where the head is arrested in the cavity, and as a corollary it is urged that in cases of arrest at the brim, where the head has been brought into the cavity by the long forceps, this instrument, after serving so far, should be discarded and replaced by the short forceps. I do not concur in this view. I doubt whether any one who has had any considerable practice with the long forceps has found it worth while to change instruments in the course of delivery. The long forceps possesses a more scientific adaptation to the pelvis throughout the whole canal than the short forceps. And if the long forceps is found in practice capable of taking the head through the pelvis from brim to outlet, it follows that, since the whole contains the parts, the long forceps is qualified to take up the head at any point below the brim.

The pelvis has been compared to a screw. I think a better idea may be formed of its mechanical properties by comparing it to a rifled gun, and the child's head to a conical bullet. But even then the comparison is not complete, for the pelvis, unlike a gun, is a curved tube. Now, just as the head must traverse the pelvis in a helicine course, determined by the relation of form between pelvis and head, so is it natural that an instrument designed to grasp the head should be so modelled as to be fitted to follow this helicine course during introduction and extraction. This indication a well-modelled long forceps fulfils; no single-curved forceps can fulfil it.

First, as to the application when the head is delayed at the brim.

Mode of Applying.—Position of the Patient.—The patient should lie on her left side, the knees drawn up towards the abdomen; the head should be only slightly raised. She should lie across the bed, with the nates near the right edge, about midway between the head and foot. This will facilitate the introduction of the blades, and give room for the sweep of the handles round the pubes at the end of the operation. I do not find it necessary to bring the nates to hang over the edge of the bed. I have often passed both blades when the patient has been lying in the middle of the bed. Sometimes it is very desirable to move her as little as possible.

First Act—Introduction of the Blades.

Selection of the Blades.—First dip them in warm water, wipe dry, and lubricate with oil, or lard, or cold cream. Join them, and, holding the instrument with the concavity of the pelvic curve forwards, and the blades in the position which they are to occupy in the pelvis, you take that one first which is to lie in the left or lower side.

First Stage.—One or two fingers of the left hand are passed inside, in at the perineum and between the cervix uteri and the head. Then, bearing in mind the relative forms of the instrument, the head, and the pelvic canal, the point of the blade is passed along the palmar aspect of the fingers at first nearly directly backwards towards the hollow of the sacrum.

Second Stage.—The handle is now raised so as to throw the point downwards upon the left side of the head. As the point of the blade must describe a double or compound curve—a segment of a helix—in order to travel round the head-globe, and at the same time to ascend forwards in the direction of Carus' curve so as to reach the brim of the pelvis, the handle rises, goes backwards, and partly rotates on its axis.

Third Stage.—The handle is now carried backwards and downwards to complete the course of the point around the head-globe and into the left ilium. Slight pressure upon the handle ought to suffice. This will impart *movement* to the blade; the *right direction* will be given by the relation of the sacrum and head.

The blade is now *in situ;* the shank is to be pressed against the coccyx by the back of the operator's left hand whilst he is introducing the second blade.

Introduction of the Second Blade.

First Stage.—Two fingers of the left hand, the back of which is supporting the first blade against the perineum, are passed into the pelvis between the os uteri and the side of the head which lies nearest to the right ilium. The instrument held in the right hand lies nearly parallel with the mother's left thigh, or crossing it with only a slight angle. The point of the blade is slipped along the palmar aspect of the fingers in the vagina, across the hank of the first blade *in situ,* inside the perineum towards the hollow of the sacrum.

Second Stage.—As the point has to describe a helicine curve to get round the head-globe and forwards in the direction of Carus' curve, the handle is now depressed and carried backwards until the blade lies in the right ilium. When it has reached this position the handle will be found near the coccyx, nearly in opposition to the first blade.

The Locking.—This is effected by a slight movement of adaptation. A handle is seized in each hand. The handle of the first blade is brought a little forwards over the handle of the second blade. If one blade is a little deeper in the pelvis than the other, it is either brought out, or the other is carried in until the lock is adjusted. This is commonly facilitated by pressing both handles backwards against the coccyx. This movement, by throwing the blades well into the ilia, where there is room, allows the handles to be rotated a little, so as to fall into accurate relation.

Accurate locking is generally evidence that the blades are properly adjusted to the head, and that the pelvis admits of the successful use of the instrument. On the other hand their not locking is proof of their not being properly introduced, or *of the pelvis not admitting of their application.* In the first case, that of improper introduction, the failure is generally due to neglect in passing the blades exactly in the same diameter of the pelvis—that is, in passing the second blade exactly opposite to the first, so that if the first blade is applied to the left ilium, opposite one end of the transverse diameter, the right does not lie at the opposite end of that diameter. To remedy this error, the blade must be partly or wholly withdrawn and re-adjusted.

In the second case, that of pelvic unfitness, the locking is prevented by the projecting promontory or other deformity, so distorting the pelvic diameters that the two blades cannot find room to lie in the same diameter opposite to each other.

It will commonly be found that the blades will pass one on each side of the promontory, the inside of the blade not looking towards its fellow, but towards the opposite foramen ovale, where you cannot get a blade to lie. When you find this happen you must give up the attempt to use the forceps. Pass the hand into the pelvis, if necessary; explore its dimensions and form carefully; and determine between turning and craniotomy. A correlative proposition may here be stated :—*Wherever the long forceps will lock without force, it may be reasonably concluded that the case is a fit one for the trial of this instrument; and a reasonable attempt should be made to deliver by its aid before passing on to turning or perforation.*

3. *The Extraction.*—Get the nurse to press upon the right hip and support the back. Grasp the handles with one hand, and apply the fingers of the other hand to the ring or shoulders at the lock. Draw at first backwards in the axis of the brim, during the pains if any be present, and at intervals of a minute or so if there be none. Concurrently with traction, alternate slight leverage movements may be executed by swaying the handles gently from side to side, always taking care not to press the shanks against the pelvic walls. Each blade is the fulcrum to its fellow. The finger which is used in the ring from time to time gauges the advance of the head.

The advance of the head is measured by the following standards :—First, you feel if the occiput approaches the pubic arch by passing a finger below and behind the pubic bones. Secondly, you sweep your finger round the circumference of the brim, and thus feel if the equator of the head-globe is pressing lower down through the brim. Thirdly, by feeling the direction of the sagittal suture. If you find that it is approaching parallelism with the conjugate diameter, you may be certain that the head is descending. Further evidence is found in the rotation of the forceps. As the head can hardly turn upon its cervico-vertical axis without at the same time descending in the pelvis, if the handles of the forceps are observed to rotate, this rotation, being imparted by the head, is evidence of advance. Again, as the head descends, of course more and more of the shanks and blades will become visible. This, indeed, is open to a fallacy. Allowance must be made for some degree of slipping, which takes place with all the English instruments whose blades have only a moderate bow. And further, when the head is fairly in the pelvic cavity, the blades lose something of that external support which, is the chief force in maintaining the grasp upon the head. This is still more marked when the head has partly emerged from the vulva. At this time the the blades will be apt to slip away altogether, and it will be necessary to increase the compression on the handles in order to keep your hold. Fourthly by two or more fingers you measure the space or degree of tightness between the vertex and the floor of the pelvis. At first the fingers find free space; gradually the vertex leaves no room for the fingers. Then the soft floor of the pelvis, the perineum, is distended by the advancing vertex; it bulges out; it puts the perineum tightly on the stretch. The anus is protruded. Fæces are often squeezed out. Indeed, the pressure upon the sphincter ani at this stage sets up reflex action. The call to strain or bear down to expel the pelvic contents, whether uterine or rectal, is uncontrollable. Turbulent expulsive action, then, and defecation, constitute certain signs that the head is advancing. To some extent the increasing scalp-swelling or caput succedaneum may give a false impression that the cranium itself is descending. But a little practice and attention will correct this error. When the vertex has reached the floor of the pelvis, the handles of the forceps are found to have turned a little upon their axis, to lie more nearly in the transverse diameter of the pelvis. This is the result and the indication of the screw-rotation of the head. You have no hand in producing it. It is effected by the descending head adapting itself to the cavity of the pelvis. The handles may now be directed more forwards during traction. The shanks thus avoid stretching the perineum, and the traction is in the axis of the outlet. An assistant is now useful in holding up the right knee, so as to leave room for the operator to carry the handles well round the pubes in Carus' curve. Here it is often convenient to push the handles forwards rather than to pull.

During extraction it occasionally happens that the blades will lose their hold, that the handles will twist in opposite directions, and thus unlock. This is generally owing to the operator carrying the handles forward too early. The effect of this is to throw the blades off the head-globe over the face. It is another illustration of the law that the position of the forceps is determined by the relation of the head to the pelvis, and that if you reverse the order by attempting to make the forceps alter this relation you are immediately at fault. The remedy is to carry each handle well back again towards the perineum, when they will re-lock.

If the head is in the genital fissure, and there is sufficient uterine energy, you may proceed to the

4th Act. The Removal of the Blades.—If the head should not be propelled, you may often assist it by a manœuvre which it is well to understand. You apply the palms of both hands one on either side and behind to the perineum distended by the head; and bearing upon this structure so as to press it a little backwards, whilst the head is pushed forwards towards the pubic arch, the head is, as it were, shelled out by being made to complete its movement of extension. Steady pressure by the hands of an assistant or by a binder upon the fundus uteri will much assist the extension of the head. In this manner I once extricated myself and my patient from an awkward predicament. I had been summoned into the country without knowing the nature of the case, and had no instruments. I found a lady who had been many hours in labour, the head on the perineum, and no pains. The lever or the forceps would have delivered her in a minute. Neither was to be had. But the manœuvre I have described perfectly succeeded, and put an end to a state of extreme anxiety, and even danger.

Another manœuvre is occasionally serviceable. This is to pass a finger into the rectum, so as to get a point of pressure upon the forehead. In this way it is sometimes possible to bring the face downwards, to start the extension movement, and thus to extricate the head delayed at the outlet. And if at the same time firm downward pressure be made upon the breech through the fundus, the force propagated through the spine will aid materially in giving the extension movement. This combination of the principles of "pushing," of leverage, and of "shelling-out," may in certain cases enable you to deliver without resorting to the forceps or lever.

When the blades are adjusted, they will not lie exactly in the transverse diameter of the pelvis. The head, lying between the transverse and right oblique diameter, will tend to throw off the blades towards the opposite or left oblique diameter. The head then will be seized obliquely, one blade grasping the right brow, the other the left occiput. This is clearly demonstrated by the impressions of the fenestræ left on the scalp. The blades naturally find their way into this position if they are introduced gently. One tendency of this oblique seizure is to assist the head in its axial rotation, face sacrumwards, as it descends into the pelvis. It is also an answer to an objection urged against the use of the long forceps at the brim—namely, that by seizing the head in its long or fronto-occipital diameter, compression in this direction makes the opposite or bi-parietal diameter bulge out, thus increasing the difficulty of passing the small or conjugate diameter of the pelvis. In most cases the objection is theoretical only—it is mainly based upon experiments made on the dead fœtus on the table.

Elongation or moulding, we have seen, is the result of gradual compression of the equatorial zone. Now the pelvis and the forceps together constitute the compressing ring. Pressure, then, upon the transverse diameter of the head by the opposing points of the sacrum and pubes, simultaneously with pressure upon the longitudinal diameter between the blades of the forceps, tends to *diminish both diameters* by lengthening out the head. Of course it must be understood that the pelvic contraction is of moderate degree only—in short, that the case is a proper one for the forceps. If the conjugate diameter be less than 3·25 inches, the prospect of effecting the desired elongation within a reasonable time is greatly diminished.

I have said that the head is very rarely seized exactly in its longitudinal diameter. An exception occurs in the case of the very flat pelvis, in which there is conjugate contraction with very little projection of the promontory. In this case the head will lie very nearly in the transverse diameter. If, in presumed contraction of the brim, the marks of the blades are on the brow and side of the occiput, the projection of the promontory is not great.

The Time required for Extraction.—If the head be delayed in the cavity of the pelvis for want of expulsive action, or because it rests upon the ischia, maintaining a too near approach to the transverse diameter, and there is no marked hindrance on the part of either the anterior or posterior valve, it is generally sufficient to use slight traction and oscillation for a few minutes. As soon as the head is started by the forceps, the uterus takes up its work, helps the operator, and the labour is quickly over.

If the uterine and perineal valves obstruct the passage of the head, a little more time and caution are required.

If the head has to be seized at the brim on account of delay from want of uterine action, time may often be saved by placing the patient on her back, and supporting the uterus against the spine by the hands of an assistant or a binder. This proceeding, by adjusting the axis of the uterus to that of the brim, will greatly facilitate the entry of the head and encourage the action of the uterus. If there is no obstacle from narrowing of the pelvis or want of dilatation of the soft parts, gentle traction and oscillation during ten minutes will generally complete the labour.

In the event, however, of arrest from pelvic contraction or from want of dilatability of the soft parts, time is a necessary element. The process of moulding, of elongation of the head, can only be effected gradually. Here oscillation or leverage must be used with great care. What is wanted is steady compression and traction extended, with moderate intervals of rest, over thirty minutes, or even an hour. Should the head be found to make no advance in entering the brim in that time, the question whether the forceps must not be laid aside for turning or perforation will have to be considered.—*Medical Times and Gazette, July* 27, *Sept.* 7 *and* 21, 1867, *pp.* 85, 249, 313.

99.—ON DR. BEATTY'S MIDWIFERY FORCEPS.

By Dr. THOMAS EDWARD BEATY, Dublin, and Dr. ROBERT BARNES, London.

[Dr. Beatty considers that Dr. Barnes has fallen into a mistake in describing this instrument, in his lecture published in the Medical Times and Gazette for Aug. 24.]

In speaking of the short forceps, Dr. Barnes remarks, "The best short forceps is perhaps that of Dr. Beatty, of Dublin. It much resembles the short forceps of Smellie. I used it for some time, but I have given it up because of these two faults, and its inadequacy to cope with a large range of cases which come within the power of the long forceps." From this description I can only conclude that the instrument spoken of is not mine at all, for it does not resemble that designed and published by me twenty-five years ago, and reprinted in my recently published volume, "Contributions to Medicine and Midwifery."

My forceps are not short, but were designed as intermediate between the long clumsy French forceps and the old short ones of Smellie. A pair of the latter in my possession, and which I used before I contrived the longer instrument, measure ten and a half inches in the entire length, of which the blades measure six inches. My forceps, spoken of by Dr. Barnes as resembling that of Smellie, is thus described at page 116 of the volume of "Contributions" just alluded to:—"The entire length, including the handle, is twelve and a half inches; of the blade to the lock, eight inches; of the fenestrum, five and a quarter inches; the greatest breadth of blade, one inch and three-eighths; the widest part of the fenestrum, one inch; the distance between the blades when joined, three inches at widest part; distance between the extreme points, one inch and one-eighth; weight of the whole, ten and a quarter ounces. The sides of the blades enclosing the fenestra are nearly round, slightly flattened upon the inner and outer surfaces, but having no sharp edge either upon the outer border or the margin of the fenestra." From this it will appear that my forceps are well beyond the limit to which the term short can be applied, and that the resemblance to the instrument of Smellie is very remote. But on looking at the description of the instrument recommended by Dr. Barnes, and comparing the measurements of it with those of my own, quoted above, I find that there is a much greater resemblance between them than between mine and Smellie's. In fact, in the blade, the most important part of the instrument, mine is an inch longer than that adopted by Dr. Barnes. In the latter he describes the length of the blade to be seven inches—in mine it is eight. His handle is five inches, while mine is four and a half. In Dr. Barnes's forceps the length is eked out by what is called a shank intervening between the point where the blades begin to separate and the lock, the length of which shank is not mentioned.

I trust it will not be considered that I am encroaching too much on the valuable space of this journal if I make a few observations on Dr. Barnes's two objections to straight or single-curved forceps, which mine are.

Objection 1.—"To introduce the second or upper blade, the handle must be depressed nearly at right angles with the mother's left thigh, which is flexed upon her abdomen. Now, to do this, the patient's nates must be dragged over the edge of the bed; to procure and maintain this position is often a matter of great difficulty and inconvenience."

Objection 2.—"In extraction, the handles nearly to the last moment must be directed more backwards than is necessary with the double-curved forceps, and owing to the bows springing directly from the lock, the perineum is wedged open, and not seldom unavoidably torn. In some cases this injury may be avoided by taking off the blades before the greatest diameter of the head passes, but then the work is not always done, and you have to put them on again."

Now, as to the position of the woman to enable us to pass the second blade of the straight forceps, it differs in nothing from that required to pass the first, or a double-curved blade, if such be used. It is the well-known obstetrical position in which we place our patients in ordinary labour in the last stage. It is that in which she must lie for catheterism, in which the operation of turning is performed, and which she must observe when craniotomy is necessary. And when it is recollected that delivery by the forceps is usually performed under the influence of chloroform, when we can place and maintain our patient in any position we like, this objection ceases to have any force. But in the application of my forceps it is of no moment, for the depression of the handle nearly at right angles with the mother's thigh is not only unnecessary, but wrong.

I think the best comment I can make on this, and the second objection just quoted, is to ask Dr. Barnes to be kind enough to open my volume of "Contributions" at page 118, where, in reference to this very subject, I make the following observations:—

"From having at times seen awkward attempts made to introduce the blades, I am induced to add a few remarks upon the method of using the instrument. The point should never be urged forward in the direction of the long axis of the blade. Such a proceeding is calculated to inflict injury on the mother, if the blade is sharp on the outer edge, as it is in most forceps found in cutlers' shops. On the contrary, the operation should be commenced by laying the concave surface of the blade flat upon the lowest part of the child's head, and when so placed *the handle will project between the thighs of the patient*; then bringing the handle slowly down, and, by a series of lateral sweeping movements, during which the point is at the same time slowly passed over the side of the head, describing on it a curved line, the blade is coaxed, as it were, into its proper place. No force should be ever used; if any resistance is encountered, it is an indication that some error has been committed. Better to stop, and even withdraw the blade and begin afresh, than use violence to urge it forward. When one blade is thus placed, the handle will be found to lie close to the fourchette; it should be given to an assistant to hold steady, and then the same proceeding should be adopted to ensure the safe introduction of the other. Having got the head fairly embraced by the instrument, the extracting force should be applied in the direction in which the handles point. If the head be high up in the pelvis, they will be found hard pressed against the fourchette, and pointing backwards. In that direction, then, the handles must be drawn in the first instance, and, as the head is moved, the handles come more directly downwards; in which case the direction of the force is to be changed, still following the rule that it must be used in the direction of the handles. By degrees, as the head comes to rest upon and distend the perineum, the handles will point forwards, and finally they will be found between the thighs of the patient, in which position the last part of the delivery is accomplished. This quality of pointing out by necessity the axis of that part of the pelvis in which the head is lodged, and thus enabling us without fail to use our extracting force in the most suitable direction, gives a great superiority, in my mind, to the straight blades over those with the lateral curve, which are so much used (in England and Scotland). The latter, from their curved form, give an erroneous idea of the position of the head, and may cause the expenditure of much unavailing force in a wrong direction."

I have only to add that the narrowness of my blades, they being only one inch and three-eighths broad, together with the elongated curve which they describe, render their introduction and application, in the most difficult cases, a matter of very easy performance.

I am sure Dr. Barnes will excuse me if I make use of one of his own diagrams to substantiate the opinions and precepts set forth in the passage just quoted. Figure 13, in his third lecture, p. 196, is what I would have drawn if I wanted to demonstrate the superiority of my straight forceps over that with the double curve. The head is represented in that diagram in a position that very often demands delivery by the forceps. The greatest portion of it has passed through the brim of the pelvis, but the lower surface has not come to touch the perineum, and the hollow of the sacrum is still unfilled up. It will be manifest to anyone looking at that diagram that the short forceps is well applied to the head, and that the first direction in which that head must move is backwards and downwards until it comes to occupy the hollow of the sacrum. The short forceps are placed precisely in that direction, and extracting force exerted in the direction pointed out by the handles is what the case requires in the first instance. If traction be thus employed, it is manifest that the bulk of the head will be drawn into the concavity of the sacrum, the chin will be removed from the breast of the child, and as this takes place the handles of the forceps will gradually come forward, and as the head descends will follow the curved dotted line on the diagram until, the head having rotated on an imaginary axis running through the parietal protuberances, the face comes to occupy the hollow of the sacrum, and the occiput protrudes beneath the pubes. Now, if we examine the position of the long curved forceps, as represented in the diagram, it will be manifest that traction cannot be made in the proper direction—viz., backwards and downwards. If force be used in the direction of the handles, the effect must be to draw the occiput down on the symphysis pubis, which will of course resist its further descent, giving rise, as I have already stated, to "the expenditure of much unavailing force in a wrong direction." If my straight forceps had been placed on that head instead of the short ones that are there delineated, the contrast would have been still more strongly marked.

The last objection that Dr. Barnes makes to my forceps is "its inadequacy to cope with a large range of cases which come within the power of the long forceps." I have already stated my belief that Dr. Barnes never had a true copy of my forceps, and this observation is quite conclusive upon the matter, for the ease with which heads can be seized when they have scarcely entered the brim of the pelvis, and the facility given to their extraction by the shape of the blades, are points dwelt upon by all who use the genuine instrument. Any one who will take the trouble to look into my volume above alluded to will find abundant evidence of the power of my forceps to cope with every case. I will content myself with the following extract:—

"As an illustration of what can be done with the forceps above described, I will quote one case from my case-book (page 120).

"September 16, 1859.—A lady, tall, thin, and delicate, eight months pregnant for the third time, living three miles from Dublin, had taken a house in town by my desire for her confinement, and was in the act of getting into her carriage to come to town when she was seized with a convulsion. This was at about 12 o'clock in the day. I was sent for, and reached her as soon as possible. Her former labours had been natural. I learned that for a week previous to the present attack she had been complaining of headache; I found her after a second fit quite collected and free from pain in the head. I examined and found no sign of labour. The convulsions returned every hour, increasing in severity, but with intervals of complete consciousness. At 9 o'clock p.m. there was some slight labour pain, but still no dilatation of the os uteri. At 11 o'clock the os was open to the size of a shilling, and the convulsions still kept recurring every hour. At 12 o'clock she had another bad fit, when I found the os uteri about as large as a two-shilling piece and soft. I tried dilatation with my fingers, and succeeded in enlarging the opening to such an extent that, if it had been desirable to turn the child, one would have been able to force the hand into the uterus.

"The head of the child lay well down on the cervix uteri. A moment's reflection convinced me that if I could get my hand into the uterus I could as well introduce the blades of my forceps, and accordingly, having ruptured the membranes, I proceeded to pass the instrument, first one blade, then the other, along the sides of the head, and I readily closed the handles. Then, by slow traction, I drew the head through the os uteri and into the pelvis, and delivered a live boy, who is now seven years old, strong and healthy. I think an instrument capable of accomplishing a feat such as this does not require any further commendation."

In conclusion, I will observe that the old short forceps is totally abandoned in Dublin, and that for many years no forceps has been used in the Lying-in Hospital of this city but my straight forceps. I have used no other myself for twenty-five years, and I believe the instrument is very generally employed by other practitioners. We do not meet the torn perineums that Dr. Barnes fears as the result of its use, and those who use it the oftenest are disposed to prize it the most.

[Dr. Barnes replies to Dr. Beatty as follows :—]

I have read with great pleasure, and I trust with profit, the criticism of Dr. Beatty upon my objections to his forceps. That criticism, at once judicious and generous, as might be expected from a man so eminent for experience and kindly courtesy, impels me to offer some further observations upon the subject.

1. I have said that "the best short forceps is, *perhaps*, that of Dr. Beatty, of Dublin. It *much* resembles the short forceps

of Smellie." I would, at the outset, make two corrections—by striking out the words "*perhaps*" and "*much.*"

2. The term "short," to which Dr. Beatty objects, also requires explanation. I use it as alternative with "single-curved," as distinguishing this class of instruments from the "double curved," or "long" class. It would be more accurate to discard the terms "long" and "short," for a single-curved forceps may be "long," as is the case with Dr. Beatty's, and a double-curved one may be "short," as are some that I have seen.

3. As to the resemblance of Dr. Beatty's forceps to Smellie's short forceps. Here further explanation is necessary on my part. Dr. Beatty's instrument is considerably longer than Smellie's. The resemblance consists in the general form of the blades and fenestræ, and in a great measure *in the use to which both can be applied.* This use is essentially that to which the single-curved forceps is nearly entirely, and, in my opinion, properly, restricted—namely, the application to deliver a head wholly or partly engaged in the pelvis. I have, of course, discharged my duty and gratified my desire for enjoyment and profit by studying Dr. Beatty's "Contributions to Medicine and Midwifery." I find in his excellent and most useful memoir on the forceps these words :—"In the observations that follow . . . I will allude only to the more common cases of difficult labour, in which, after several hours of severe pain, *the head has been forced to a greater or less extent into the cavity of the pelvis*, and is there arrested." (p. 97.) And in the histories of the fifteen cases narrated, it is stated in every instance that the head was partly or wholly in the pelvis. At p. 116, it is said that, "from the length of the blades, the lock will not be within the vagina." This, to my mind, is further evidence that Dr. Beatty does not insist upon the fitness of his instrument to seize and deliver a head arrested *above the pelvic brim.* I have tried his instrument in such cases, and the lock had to go fairly within the vagina. Being longer, however, Dr. Beatty's forceps will command a greater number of cases than Smellie's, and yet it leaves many cases behind which a perfect forceps ought to command.

4. I will now say a few words on Dr. Beatty's reply to my "Objections." First, as to the necessity, alleged by me, that the patient's nates must be dragged over the edge of the bed in order to allow the handle of the upper blade to be sufficiently depressed to enable the point to travel over the head-globe. I contend that in certain cases—I have met with them—as of convulsions, hemorrhage, great restlessness of the patient, faulty structure of the bed, and the want of assistants, it is difficult, highly inconvenient, or even impossible, to get or

to maintain the patient on her left side, with the nates well over the edge of the bed. I have had to deliver by forceps a woman in convulsions on the floor. In such cases the advantage of possessing a forceps like my "double-curved," and of using it as I describe, is very great. It can be applied whether the patient be on her back, or on her side in the middle of the bed. Dr. Beatty seems to claim a similar quality for his forceps, when he says the position on the side over the edge of the bed "is of no moment, for the depression of the handle nearly at right angles with the mother's thigh is not only unnecessary, but wrong." He then describes how to introduce his forceps. "The operation should be commenced by laying the concave surface of the blade flat upon the lowest part of the child's head, and when so placed *the handle will project between the thighs of the patient;* then bringing the handle slowly down. . . . When one blade is thus placed, the handle will be found to be close to the fourchette; it should be given to an assistant to hold steady, and then the same proceeding should be adopted to insure the safe introduction of the *other.*" This is a description of the mode of introducing the pubic or upper blade, which, I believe, it is the Dublin practice to introduce first. It is the pubic blade which makes the difficulty. Now, I freely admit that in the first stage you may pass the blade into the vagina to touch the child's head in the way described, so that the handle shall be between the mother's thighs. So far you may, indeed, pass the blade in this or in any way that may be most convenient. But in the next stage, that in which the point of the blade has to travel over the head-globe, it appears to me axiomatically necessary that the handle must be depressed just in proportion to the elevation the point must take to rise over the head. Dr. Beatty himself directs "the handle to be brought slowly down." He does not indeed say how far down, but this is indicated by the conditions of the case. The point must, to get over the equator of the head, run nearly vertically upwards; the handle, therefore, must be turned nearly vertically downwards—that is, across the mother's right thigh. And the length of the forceps, "twelve and a half inches," must carry the end of the handle into the bed-clothes if the nates are not drawn clear over the edge. Is not this matter of common experience? I confess I am unable to understand how to avoid this dilemma.

5. Dr. Beatty "concludes that the instrument spoken of by me is not his at all." This forces upon me the welcome suspicion—which I am sure Dr. Beatty never dreamed of suggesting—that he has provided a good instrument, but I have not known how to use it. Accepting this alternative, I will humbly follow Dr. Beatty's instructions, and try again. I must, how-

ever, plead that I have used this instrument in at least twenty-five specified cases. In most of these cases I was greatly satisfied with its efficiency. It is admirably light, well-balanced, elegant, and very easy of introduction. Its length makes it a good lever, and I am glad to observe that Dr. Beatty recognises the principle of using the forceps as a lever. But I would also observe that owing to the shortness of the handles—the absence of shoulders or ring reducing it to a one-handed instrument—the slimness of the shanks and blades, so great that these easily "spring" or bend under compression, and the length of the radius of the bow, the grasping, compressing, and traction powers are not great. This Dr. Beatty and the adherents of the single-curved forceps will probably regard as a merit, and so it is for a forceps whose duty is limited to delivering a "head arrested in the pelvis." Moderate leverage power is often all that is wanted in such cases. But the instrument fails before those more difficult cases of arrest of the head *on the brim*, whether inertia or slight pubic contraction, I say this from ample experience, having in several such cases first tried Dr. Beatty's instrument without success, and then succeeded readily with the long double-curved forceps. The case narrated in detail by Dr. Beatty was an eight month's child. I admit that Dr. Beatty's forceps will often prove sufficient in cases of arrest of the head only partially engaged in the brim; and even in some cases where the head is quite above the brim; but then the lock runs into the vagina, power is lost, the perineum—I insist upon this—is put unnecessarily on the stretch, for in proportion as the point is directed forwards to get above the brim, the handles must be forced back against the perineum and coccyx. These inconveniences and others are quite obviated by my double-curved forceps.

6. The shank intervening between the lock at the commencement of the bow of the blades, adding two inches and three-quarters to the length of the handles—of which Dr. Beatty speaks a little slightingly—has these great uses: it carries the lock clear out of the vagina, even when the head is grasped above the brim; it increases leverage and traction powers; in conjunction with the pelvic curve of the blades, it obviates all strain upon the perineum; and by adding to the length of the handles, and supplying a ring for holding, it makes a *two-handed* instrument, to the great increase of power, delicacy, and comfort in use. I will only further remark upon Dr. Beatty's plea that in Dublin "we do not meet the torn perineums that Dr. Barnes fears as the result of its use" (Dr. Beatty's forceps) by citing a statement lately made in my presence by Dr. Denham at the Rotundo Hospital—namely, that they there had to perform the operation for lacerated perineum very frequently. Of course,

I do not imply that this necessity always arises from the use of Dr. Beatty's forceps. But the risk of tearing the perineum with *any* straight forceps, *the bow of whose blades springs direct from the lock*, even if the handles are carried well forwards, must be serious. The blades will form a double inclined plane or wedge beyond the head. No ingenuity short of taking off the instrument will always avail in preventing this wedge from splitting up the perineum, especially of a primipara.

In conclusion, I feel bound to say that, thirteen years ago, when on a visit to Dublin, I was so struck with the elegance of Dr. Beatty's forceps that I adopted it in practice, the particular specimen I used having been recommended to me by Dr. Mc Clintock. It would have given me sincere pleasure to find such an instrument do the work—I mean *all the work*—that came before me; but I soon discovered that it was not equal to the exigencies of my practice. I wanted an instrument that would more largely supplant the perforator—that would save more children, and rescue the mother more quickly from the dangers of exhaustion. I therefore fell back upon my "two-handed double-curve."—*Medical Times and Gazette, Sept.* 21 *and* 28, 1867, *pp.* 314, 342.

100.—ON DR. BEATTY'S FORCEPS.

By Dr. Henry E. Eastlake, Welbeck-street, London.

[The object of the following article is to show the vast superiority of Beatty's forceps over those which possess a double curve, in cases where the fœtal head is placed in the occipito-posterior position.]

The very fact that the natural mechanism of these cases during parturition is somewhat more complicated than those of the occipito-*anterior* variety and that the expulsion of the head is, as a rule, more slowly effected, suggests the idea that instrumental aid may not unfrequently be required in this special class of cases to which I allude. At any rate, it cannot be denied that an accoucheur is every now and then called upon to apply the forceps in cases where the child's face is directed anteriorly; and in order to advocate the special advantages of the straight forceps under these circumstances, I cannot do better than quote the expressions of Sir James Simpson upon this important point. I am led to do so from the fact that neither Dr. Beatty nor Dr. Barnes alludes to it as an advantage in the discussion which has appeared in the Medical Times and Gazette of the merits and demerits of their respective instruments. In the first volume of his "Obstetric Memoirs, " Sir

James writes: "*In occipito-posterior positions, the mechanism of the extraction of the head with the forceps should be an exact imitation of the mechanism of the expulsion of the head by nature.* In other words, I am strongly convinced that, in the artificial extraction of the head in occipito-posterior positions, we should make the forehead rotate backwards, and the occiput forwards, according to those rules which we have seen nature following under the same conditions. For here, as elsewhere, the more perfectly we imitate her principles, the more perfect will our own practice be. . . . I have now happened to be called to several cases of occipito-posterior positions, in which the forceps had been applied with the greatest adroitness and dexterity; where, subsequently, every allowable degree of force had been employed, but employed in vain, to pull forth the head in its original position, with the forehead directed anteriorly; and where I have succeeded, with a tithe of the power previously used, by adding to the requisite act of extraction a simultaneous act of rotation of the head, so as to turn the occiput anteriorly and to the right, and the forehead posteriorly and to the left." The Professor goes on to state that so long ago as 1745 Smellie became aware of the importance of *rotating* as well as extracting the head, "in a case in which the large fontanelle was at the left groin." He then finishes his paper on this question with the following remark: "It is in occipito-posterior positions, above all others, that we see the superior advantage of employing a *straight* pair of short forceps, such as those of Dr. Denman or Dr. Ziegler. They enable us to rotate the head easily and safely, as I can testify to you from sufficient experience. If we employed a *curved* pair in this position, and tried to turn the head with them, we should be obliged either to introduce them at first, or extract them at last, with their concavity, instead of their convexity, looking backwards, and consequently with great and unnecessary risk of contusion and laceration of the soft structures of the mother from the projecting ends and sides of the blades."—*Medical Times and Gazette, Oct.* 12, 1867, *p.* 417.

101.—ON THE CEPHALOTRIBE.

By Dr. J. Braxton Hicks, F.R.S., Examiner in Midwifery at the University of London.

[From a consideration of the cases in which he has used the instrument Dr. Hicks regards it rather as a substitute in the severe forms of obstruction, for the other craniotomy instruments after perforation; and not so much as a substitute for Cæsarian section.]

It is, no doubt, very valuable both in doing away with the danger arising from spicula of bone, and from the crotchet, &c., as well as tending to shorten the duration of the operation; but, if we rely upon it as an instrument capable of accomplishing the whole delivery by itself, we shall be overrating it, and exposing ourselves to disappointment. For it is not a tractor; and therefore, should pains be absent, we shall generally have to draw down the head by some other means. When it is regarded as a substitute for the Cæsarean section, it seems that the advantage of removing the calvarium, and inducing face-presentation, as recommended by me in a paper in the *Transactions of the Obstetrical Society of London*, Vol. vi, has not been fully recognised; because I believe that there are very few cases of contracted pelvis in which we cannot as certainly deliver by this means as by the help of crushing, though perhaps not so readily. Hence the comparison rests rather between these two plans, than between the cephalotribe and Cæsarean section; for the latter would be reserved for a more complete obstruction.

I am here, of course, discussing the subject on the assumption that we are acting under necessity (for instance, where the child is dead). Where we are employing the Cæsarean operation as one of election, other considerations enter. I think that, by means of this instrument, or the plan to which I have alluded, we can extend cephalotomy to its extreme point; so that, if the outlet be good, we can deliver in a brim with an anterior-posterior diameter of 1½ to 1¾ inches without serious risk, provided the transverse be above 3½ inches, and the fœtus of medium size.—*British Medical Journal, Oct.* 19, 1867, *p.* 337.

102.—ON THE CEPHALOTRIBE.

By Sir James Y. Simpson, Bart., Professor of Midwifery in the University of Edinburgh.

Under certain circumstances in practice, obstetricians were often obliged to reduce the size of the child's head, and for that purpose they had recourse to a variety of measures, craniotomy being one. At present that was not a very favourable operation in hospital practice. He (Sir James Simpson), having looked at Dr. Churchill's book, found that there were 310 cases of craniotomy performed in the Dublin Hospital. Out of these 65 died, or more than one-fifth. The operation was attended with great danger for various reasons; and especially for this reason, that when we perforate the child's head and take away a piece of bone, we are extremely apt to injure the vagina; and, again, by the operation, the size of the child's head was often increased. Dr. Hamilton, many years ago, was in attendance on a young woman whose pelvis was very deformed, and, in order to deliver her, he had to remove the whole arch of the cranium; and he states that after having done it, which took him eight hours, he had to be rolled up in blankets and carried home himself; so what must have been the state of the patient when the doctor was so bad? Notwithstanding all that, they had the base of the skull quite entire; and in putting on the cephalotribe it broke the head at once. In reference to the size of the pelvis, M. Pajot stated that he delivered women with a pelvis of an inch and a-half. That, in Sir James Simpson's opinion, was too small a pelvis to be delivered at all, and it was one where the woman ought to submit at once to the Cæsarean operation. He himself had attended a case where a very deformed woman was attempted to be delivered by craniotomy. He used that instrument (exhibiting a cephalotribe), and the delivery was over in a two or three minutes. The instrument used was quite curved, and the advantage of it was, that it would not slip when a straight pair would. One of the great difficulties is this, that after fixing the head it slips backwards. The instrument needs to be pushed very far forward, and with a long straight forceps it is almost impossible to hold the head. The instrument exhibited was a very powerful one and very heavy. The cephalotribe, with teeth for fixing the scalp of the child, he regarded as a great improvement; but it is a little more complicated in its arrangement than others. It was an instrument capable of crushing any head, and rendered the operation of craniotomy easy and safe for the patient.—*British Medical Journal, Oct.* 19, 1867, *p.* 337.

103.—OBSERVATIONS ON THE CONSTRUCTION OF THE CEPHALOTRIBE.

By Dr. George H. Kidd, Assistant Physician to the Coombe Lying-in Hospital, Dublin.

In the Dublin Quarterly Journal of Medical Science for February and May of the present year, I have published cases in which the operation was performed, and where there was more or less marked narrowing of the antero-posterior diameter of the pelvis. In one instance there was but one inch and three-quarters in this direction, and yet the mother was delivered safely and made a good recovery, as did the mothers in all the others, except one, where she was in a state of such extreme exhaustion when I first saw her, that I feared she would sink under any operation, and though, owing to the ease with which the head was extracted by the cephalotribe, her remaining powers were not overtaxed, she never rallied, and died in eighteen hours.

In speaking of these cases, and subsequent experience has confirmed me in the opinion, I have stated that I was convinced that the cephalotribe should be used in all cases of embryotomy in preference to all crotchets, hooks, and craniotomy-forceps, and that the use of it would reduce the dangers of embryotomy to a minimum, and allow its performance in cases where delivery could not otherwise be accomplished except by Cæsarean section, and that even though the child were known to be dead. My earliest operations were performed with Sir James Simpson's first modification of the instrument, but in the use of it I saw, from time to time, that some further changes in it were desirable, and it is my object now to show an instrument that I have found to answer all the purposes I have hitherto required. The action required of a cephalotribe is twofold :—

1st. As a compressor, to compress the head, and especially the base of the skull, so as to allow of its passing through a narrowed pelvis.

2nd. As an extractor, to hold the part to which it is applied, so as to allow of its being rotated in the smallest possible space, and of the most diminished portion of the presenting part being brought into the narrowest diameter of the pelvis. Moreover, its hold must be so secure as to allow of the use of a sufficient amount of extractive force to draw the child through the pelvis.

Authors differ as to the relative importance of the action of the instrument as a compressor and as an extractor. Hennig of Leipzig has added crotchet points capable of being projected when the instrument is applied, to increase its power as an extractor ; and Depaul aims at the same object, while M. Pajot and Dr. Hodges look on its use as a compressor as the most important, and, in extreme cases, Pajot is satisfied with breaking up the head by repeated applications of the instrument, and allowing it to be expelled by the uterine contractions. I believe myself that in a very narrow pelvis the cephalotribe should be applied and re-applied so as to break up the head sufficiently to enable it to pass through with ease and safety, and that then the labour may be shortened by using a certain amount of extractive force. But, whether used as a compressor, extractor, or both, the form of the instrument should be such as to admit of its easy introduction, application, and locking, with the greatest possible facility and safety, and in the narrowest pelvis in which it could be used.

In the instrument I have had made, I have endeavoured to meet all these requirements. Instead of a blade with a pelvic curve, I have adopted a straight blade. I believe this has four great advantages. First, it permits of the head being rotated in a smaller space than with a curved blade. It seems obvious that a head seized in a curved instrument must, when rotated, either turn on the axis of the handle of the instrument or on its own axis. If it turn on the axis of the instrument it will pass through a segment of a circle whose diameter will be double the length of the curve. If it turn on its own axis then the handle of the instrument must sweep through the circle. Now, as the instrument is chiefly required in cases of narrowing of the antero-posterior diameter of the pelvis, and is applied in the transverse or oblique diameter, and it is desirable that when the head is crushed in this direction it shall be turned so as to bring its crushed and shortened portion into the antero-posterior diameter of the pelvis, it is of the utmost importance that it should be capable of effecting this rotation in the smallest possible space, and this, it is evident, can only be done with a straight blade.

Secondly, it is easier to guide a straight instrument when introducing it than a curved one. In introducing a straight instrument we have but one element to consider as to its progress, whereas, with a curved blade, we have to follow with the mind's eye, not only the onward course of the point, but the peculiar direction the blade may take in consequence of its curve, and to guard against injury to the parts met with in this course; and, as it has often to be passed up far above the brim of the pelvis, the importance of this cannot be over-estimated. The pelvic curve in Baudelocque's original instrument measured three-inches and three-quarters; to facilitate the turning of the head, Braun, of Vienna, reduced it to two inches. I advocate doing away with the curve altogether.

Thirdly, it holds the head more securely. When the blades are applied, and we begin to make compression, if they rest near the posterior part of the head, it is liable to slip from between them, and so elude the grasp of the instrument. In one of my operations, in which I used Simpson's instrument, this occurred repeatedly, but was obviated by fixing the head by making pressure over the pubes, which was done by my colleague, Dr. Ringland. Cazeau and Busch tried to prevent this slipping of the head by increasing the pelvic curve, but a little consideration will show that this was a mistake, for, as the head, in the great majority of cases, lies with the face to one or other sacro-iliac synchondrosis, and the occiput directed forwards, and as the point of the curved blade is always directed forwards, the curve actually increases the risk of slipping. Schölles attributed the difficulty, more correctly, to the shortness of the blades, and consequently lengthened them, and for this and other reasons I have followed his example; but while I believe the only effectual way of preventing it is to fix the head by a hand placed above the pubes, I have no doubt it is much less likely to occur with a long straight blade than with a short curved one.

Fourthly, in making extraction, the handle of the straight instrument indicates the direction in which the traction should be applied. With an instrument having a pelvic curve, a calculation has to be made at each stage of the operation as to the direction in which the force should be applied. With a straight blade, the handle changes its position as the head advances, and the force is always to be applied in the line of the instrument, as has been ably shown by Dr. Beatty, in reference to the forceps, in his *Contributions to Medicine and Midwifery.*

The length of the blade in my instrument is ten inches, nearly equal to Baudelocque's and Scanzoni's, and greater than in most others. By this increased length the base of the skull is more surely reached, and therefore more surely broken up. The hold of the instrument is more secure, and the head less likely to slip from between the blades, and when the instrument is introduced the lock is external to the vulva, and nipping of the labia is therefore avoided. In considering the length of the blades it is to be remembered that in the cases of extreme narrowing the head is always quite above the brim of the pelvis, and the depth of the pelvis itself is often much increased. When the blades are closed, their extreme width is one inch and a-half, and the cephalic curve is formed by a curve commencing at an inch and a-half from the points. The space between the blades at the widest part when closed is one inch. The gradual cephalic curve, and the closeness of the blades, will allow of the instruments being applied in a narrower space than most others. It is true that they diminish the extracting powers of the instrument, but this I look upon as an advantage, as, for the safe use of the cephalotribe, the head should be crushed to such a degree by repeated applications that it may be extracted without force. The lock in my instrument is the reverse of that in general use. It is made so, that when the upper, or right, blade is introduced, the patient lying in the ordinary obstetric position, on her left side, the lock of that blade looks forwards. I have spoken at some length on this formation of the lock in my papers in the *Dublin Quarterly Journal of Medical Science.* I believe no change could be made in the cephalotribe, or in the midwifery forceps, that would tend more to facilitate their correct application and use than this. Formerly forceps were made in this way. I have a pair showing this, made, I believe, in 1790. In the museum of the Rotunda Hospital there is a similar pair. Dr. Radford, in his volume of Essays published in 1836, has devoted a section to this form of lock, but still instrument makers have not adopted it. Messrs. Fannin, of this city, however, who made the cephalotribe I exhibit, now manufacture forceps with the same form of lock, and I am greatly mistaken if any operator who has once used either instrument will ever willingly operate on a case with the head high up in the pelvis with one having the lock differently placed. The handle of my instrument is half-an-inch shorter than Simpson's or Hicks', the shortest hitherto in use. This, added to the increased length of the blades, tends, no doubt, to diminish its compressing power; but I have ascertained, by repeated experiments, that there is still abundant power to compress the most firmly ossified head, and the instrument gains somewhat in lightness and portability by the change. The screw used is the same as Simpson's, which is not only most simple and easy of application, but forms a very convenient and firm cross-handle for manipulating the instrument.—*British Medical Journal, Oct.* 19, 1867, *p.* 335.

104.—THE NATURE AND TREATMENT OF PUERPERAL CONVULSIONS.

By Arthur B. Steele, Esq., Lecturer on Midwifery, Liverpool Royal Infirmary School of Medicine.

An interesting report of a fatal case of this formidable puerperal complication, published by Mr. Evans in the British Medical Journal of May 25th, induces me to offer some brief remarks upon a subject always interesting to those engaged in midwifery practice.

I wish to observe, *in limine*, that the case to which I allude appears to me, from the lucid history given by Mr. Evans, to have been of such a nature—namely, accompanied with intense toxæmia, from the presence of albuminuria; and that, in all probability, a fatal issue would have taken place, whatever treatment had been adopted. Therefore, the remarks which follow will not, I trust, be regarded as a criticism upon the remedial measures employed in the particular case in question, but as suggested by a desire, if possible, to throw additional light upon some of the more perplexing differences of opinion which exist among accoucheurs upon this disease and its appropriate treatment.

First—As to general depletion by bloodletting—we have the highest authority for copious venesection as an essential part of the treatment of puerperal convulsions; and of its good effects under certain circumstances I can add my own testimony from observation. But then by others of equal repute—M. Trousseau, for example—we are forbidden to have recourse to the lancet in this affection. How can such opposite views of treatment be reconciled? The solution of the difficulty consists in a consideration of the pathology of the disease. Without going minutely into the subject, it may be sufficient to state the well-known fact, that puerperal convulsions may arise from two distinct and opposite conditions of the system—one in which excessive distension of the vessels of the brain or spinal cord, or both, produces eclampsia by compression; the other, in which, from excessive loss of blood from hemorrhage, natural or artificial, the nervous centres are drained of their blood, giving rise to convulsions from anæmia, as was first pointed out by Dr. Marshall Hall. Thus we see that bleeding in the one case is remedial, by relieving the oppressed nervous centres, and restoring the balance of the circulation; while, in the other, it only increases the evil—"the therapeutics of the disease," as tersely put by Dr. Tyler Smith, "trenching distinctly upon its pathology." (Journal of Obstetrics, page 499.)

Such, in brief, is the principle by which we are to be guided in the matter of bleeding in this disease. My own experience induces me to believe that, notwithstanding the high authority of M. Trousseau, cases of puerperal convulsions are sometimes met with, in which early and copious venesection is an essential element in their successful treatment. I am unable to appreciate the principle upon which local bleeding can be useful, except, possibly, to relieve the secondary effects of continued pressure upon the brain from a long continuance of the paroxysms, which would, I think, be more effectually *prevented* by the relief afforded in the earlier stage by reducing the quantity and force of the general circulation by venesection.

The foregoing apparently simple and conclusive statement of the question, however satisfactorily it may dispose of the difficulty in deciding when to bleed and when to stimulate, does not, unfortunately, apply to a wide range of cases, in which the state of the circulation in the nervous centres is not the main cause of the convulsions; in many of which it may be, as M. Trousseau states, the effect of eclampsia; and in others it may not exist to such an extent as to become a subject of material consideration in the treatment.

Besides plethora, and its opposite condition, anæmia, we have other centric causes of convulsions, which may exist either with or without the first two conditions. Such are the various forms of toxæmia depending upon those agencies which interfere with the proper depuration of the blood, as constipation, and insufficient secretion from the bowels caused by mechanical pressure of the gravid uterus upon the intestines; cholæmia from insufficient action of the liver, or uræmia from affection of the kidneys. In addition to all these, we have the eccentric causes of convulsions, such as emotion, irritation of the uterus and the uterine passages; and, again, irritation through the medium of more distant incident excitor nerves, as irritation of the bowels, especially the lower part of the intestinal canal, from the presence of indurated fæces, worms, or the severe action of purgative medicines, all of which have been known to produce convulsions. Gastric irritation, and even irritation of the mammæ, caused by excessive soreness of the nipple, with mammary induration, have been the cause of convulsions.

The indications for treatment in these varied conditions are too obvious to require comment; and, under such circumstances, either depletion or stimulation, if indicated at all, can only be regarded as subsidiary or secondary measures. Next to blood-letting, the most vexed question is that relating to the propriety of artificial delivery. In forming a conclusion upon this point, we must still be guided by the pathological conditions present, and their relations to the convulsions as causes. When there is reason to believe, from the absence of any other source of irritation, that the presence of the fœtus in the uterus is the exciting cause of eclampsia, delivery by the speediest method, and that least likely to add to the irritation already existing, is the manifest indication. The propriety of emptying the uterus must entirely depend upon the peculiar circumstances in each case. If the convulsions depend upon causes evidently independent of direct uterine irritation, delivery will, I believe usually (and I speak from observation on this point) produce no impression whatever upon the course of the disease. I have frequently found the paroxysms to continue unabated for some hours after delivery has been completed.

With reference to the mode of effecting delivery, the rule must be to adopt the means least calculated to increase irritation to the uterus; and certainly I cannot but think that the forceps, on that account, should always be preferred whenever their application is practicable. The passing up of the small blades of the forceps through the vagina (and os uteri, when necessary) is far less calculated to produce irritation of the incident excitor nerves of the parturient canal than the introduction of the hand and arm in the operation of turning, or the more complicated manual operations involved in craniotomy. Wherever the forceps can be applied, their use need not be, in skilful hands, more dangerous, and, in the circumstances under consideration, I believe they would be as safe, or safer, than any other method of artificial delivery.

In conclusion, I would observe that the treatment of puerperal convulsions must, like that of almost every other disease, be founded on a system of pure eclecticism. The object is, if possible, to trace the effects to their causes, and to remove those causes in the most efficient manner. I am well aware that by many the sôle origin of puerperal convulsions is thought to be uræmia, as the result of excess of albumen in the urine. This, no doubt, is a very frequent, perhaps the most frequent, cause; but I am not convinced that it is the invariable cause. I also know that practitioners of much ability and experience rely chiefly, if not entirely, upon chloroform as the remedy—perhaps I may venture to say the specific—for this affection. I have had no opportunity of personal observation in the treatment of puerperal convulsions by chloroform; but I can well understand that, in many cases, it must be most useful in rendering the nervous system less susceptible of the exciting causes of eclampsia; and, from the evidence we have in its favour, it is impossible to doubt its efficacy; nor is its judicious employment at all irreconcilable with the principles I have endeavoured to lay down as a guide to the treatment of this formidable malady.

The explanation of the apparent difficulty suggested by the author of the report from which my remarks originated—namely, that, although albuminuria exists for some time previously to the advent of labour, it is, as a rule, only when parturition sets in that convulsions occur—may, I think, be found in the fact that, for the production of eclampsia, it is necessary to have not only an exciting cause, but a special susceptibility to impression on the nervous system. This special condition appertains more or less to the female economy during the entire period of utero-gestation and throughout the puerperal state; and we occasionally meet with convulsions as a result, at various periods of pregnancy, and sometimes also after parturition. The reason why they most frequently occur during labour is, no doubt, because at that time the condition referred to acquires its greatest intensity.—*British Med. Journal, Aug.* 17, 1867, *p.* 125.

105.—CASE OF DEATH FROM ANEURISM OF THE RIGHT OVARIAN ARTERY.

By Dr. W. M. Whitmarsh, Hounslow.

On the eve of September 23rd, 1866, I was summoned to visit Mrs. B., of Lampton, who had been taken suddenly ill. Not being at home when the messenger arrived, my assistant (Mr. Samson) went in my stead, and found the patient in a state bordering on syncope, with a fluttering pulse, great prostration, and violent retching. Very little could be gleaned from her; but she complained of great pain in the abdomen. To alleviate this, he gave her some effervescing medicine, with opium, and ordered warm applications to the feet and axillæ.

Early next morning, her friends sent to inform me that she was much worse, and desired that I should call and see her immediately.

On visiting her, I found her in bed, lying on her back, her face blanched, with an expression of pain on her countenance. The surface of the body was almost cold, and the pulse hardly perceptible. The sickness had continued, and the pain in the abdomen had not been relieved; the latter returned a dull sound on percussion. She had passed very little urine during the night, but declined having the catheter used. From her sister, I ascertained that she was 37 years of age; and had been married twelve years, four of which she had spent in America. She "had never been regular," but when she menstruated the discharge was pale and scanty, and accompanied with pain. She had never given birth to a live child, but had had four miscarriages. Her occupation had been that of a laundress, at which she had worked rather hard. She felt the commencement of this attack when she was lifting a basket of clothes from a cart. It began by a feeling of faintness which compelled her to sit down; after this she was removed home.

I questioned the patient whether she had lost blood in any way. She replied she had not. I felt convinced, however, that she had; and told her husband that she must have ruptured a vessel in the abdomen, and doubtless she would not survive many hours. I ordered brandy and water with ice, and opium to relieve pain. She died six hours after my visit.

On the 26th, I made a *post mortem* examination, and found the lower half of the abdomen covered with clotted blood; so much so, that the viscera were not discernible. The coagulum weighed about three pounds. On removing it, I traced the extravasation to an aneurism of the right ovarian artery, large enough to contain a small pigeon's egg. The sac was still filled with clotted blood. I removed it with the uterus, intending to preserve them in spirit; but the friends of the woman would not let me carry them away.—*British Medical Journal*, *Aug.* 31, 1867, *p.* 177.

106.—ON SOME OF THE DISEASES OF THE FEMALE URETHRA AND BLADDER.

By Dr. J. Braxton Hicks, F.R.S., Physician Accoucheur at Guy's Hospital.

[There is a feature in these diseases, common to those of the vagina, uterus, and rectum—namely the difficulty of discovering the exact seat of the trouble, for on account of the sympathy between the different parts, the patient not uncommonly refers her symptoms to some other than the part really affected.]

As is well known, the reflex tenesmus of the rectum produced by a fissured anus is often mistaken for uterine forcing; so, likewise, irritation of the urethra often produces a bearing-down of the uterus, and sometimes even prolapsus. Whenever, therefore, a case of much tenesmus comes before you, do not content yourself with the patient's diagnosis, but inquire into its commencement, and its original seat. Some difficulty will, doubtless, present itself to you in seeking this point, because many patients forget, in the trouble of the moment, the exact mode of its commencement; and this is particularly true if the pain have changed its seat. And you will also find, even at the moment of your inquiry, a difficulty of distinguishing from the sensations described the precise spot of annoyance; whether, for instance, the pain be in the urethra, or vagina, or uterus; whether, also, the bleeding which occurs in some conditions proceed from the vagina or urethra. These points are important, and for their elucidation may require strict investigation by a thorough examination of each of the passages.

I may here also remark that diseases of the bladder and urethra are not so much under the influence of internal remedies as they are of topical, and therefore you will find that I shall lay corresponding stress upon the latter form of treatment. Not but that much can be done by opiates, rest in the horizontal posture, and avoidance of certain beverages, and the correction of any condition of the system which may give rise to irritating urine. But, on the whole, local treatment in the severe forms is much more effective, though in some works treating of this subject you will find that the local remedies are considered too painful to admit of being used, or else they are in great measure passed by, so that the class of cases under consideration has been not so prolific of laurels as many others.

In bringing before you some of the diseases of the female urinary organs, I shall begin from without, and follow the passages inwards to the bladder.

Vascular growths at meatus urinarius.—And first I shall call your attention to those generally extremely sensitive red outgrowths of membrane of the meatus urinarius, called vascular growths.

You will find them readily, on separating the labia, by their bright red appearance at the meatus, either in the form of a minute polypus, or as a broad-based elevation of the mucous membrane, partly or wholly surrounding it, or passing some distance within. If you touch this, even as lightly as possible, n the majority of cases the patient will suffer the most acute pain; while in some few there will be no very great suffering. The difference depends, I believe, upon whether the sensitive papillæ (Pacchionian bodies) are enlarged or not. The growth n all cases is hypertrophy of the mucous membrane. In some he growth of vascular element is in excess; in some (the majority), the nerve-tissue; in others the basement layer. Now, in those which are most sensitive you can without difficulty find the Pacchionian bodies hypertrophied, and very distinct. If they are removed by the snare, they will be found beautifully injected.

You will easily understand that, if these growths are so sensitive, much suffering is complained of during walking, coitus, micturition, &c. But, generally speaking, the patient very seldom is able distinctly to explain the seat of the distress. Sometimes the pain from micturition continues setting up a forcing and bearing down, with a distress about the vulva; but, as a rule, the distress is not at first referred to the "water-passage." You must not therefore expect to be assisted much by the patient herself in the diagnosis. In such cases it is better to examine by sight, where you have any pain and difficulty attending micturition. Perhaps the most marked sign pointing to the urethra which the patient volunteers is, that after the completion of micturition a drop of blood follows; but in many cases this symptom does not occur. When it does, it is important to ascertain whether the blood really comes from the urethra, or from the uterus; because straining in micturition will sometimes cause a small, and indeed often a large, quantity of blood to flow from a malignant os uteri. The indefiniteness of the patient's description as to the seat of trouble was well illustrated in a young woman of sedentary habits, who had for two years complained of uneasiness in the genitals. She could scarcely describe its nature, or where exactly it was, except that it was in front, and that when she leaned forward it hurt her. As I could make out nothing more from her description, I examined, and found one of these vascular growths at the meatus. It being of a polypus form, I removed it with a wire noose. She called two weeks afterwards to say she had entirely lost the annoyance, and I heard afterwards she had had no return of it.

As an instance of the vagueness and distress to which these growths may indirectly give rise, I may instance the case of a woman about sixty, who came to me complaining of incessant desire to micturate; severe straining; urine highly loaded with pus and mucus. To add to this, she had recently taken spirits of turpentine with a view to cure, and this doubtless had much increased her troubles. However, she told me she had very intense pain upon connexion, for her husband still retained much vigour. This, of course, required examination, when I found that she had a large and unusually sensitive vascular growth at the meatus. This in a day or two I removed, directly after which she began to lose the above named symptoms, which in the course of two weeks had passed off, the urine recovering its natural condition. In this case a great amount of the distress was occasioned by reflex irritation, at first set up by the growth, but which she herself had increased.

The treatment of these cases is simple. Where the growth is of the polypus kind, it is best removed by a wire snare—as, for instance, by one which I have described in the "Obstetric Transactions," vol. iii., p. 346. For myself I prefer this mode because you can thereby secure its total removal, which you cannot always if you endeavour to seize it with the forceps and cut it off with scissors; for then not unfrequently the growth, always of a flimsy and vascular character, breaks down, so that it is impossible to lay hold of it again. If we attempt to cut it off with the scissors only, we may be more successful; but I have seen such efforts defeated by the drawing up of the parts at the slightest touch before it can be secured by this instrument. Under these circumstances I have found the most ready mode is that of dividing with the wire noose. Of course we may employ the galvano-cautery; but this apparatus is rather more troublesome to prepare. As a rule, after removal I prefer touching the part with a caustic, such as nitric acid, in the manner described below for the broader-based forms. Little or no bleeding ensues; that which does occur is readily staunched by pressure. In the more sessile forms I prefer the use of nitric acid, as a convenient agent, in the following manner:—Take a piece of soft wood, such as deal; nothing is more at command than a lucifer match. After cutting off the inflammable part, and rounding off the corners, this is dipped in nitric acid, and the superfluous quantities allowed to drain off, so that the wood should only be saturated with it. It can then be applied to the growth without any fear of its running over the parts not affected. Nitrate of silver is not to be relied on to destroy the base of the growth. Either the galvano-cautery or nitric acid may be used, so far as I have found, with the greatest amount of certainty.

Now these growths have a decided tendency to return, par-icularly if the growth has not been thoroughly destroyed or emoved in some manner. Therefore in any case it would be well not to promise a radical cure too decidedly. I have seen girl in whom the whole meatus was surrounded by a growth f considerable exuberance, and in whom the disease returned hree times, which was however, ultimately cured. These cases re certainly not malignant, but are rather to be considered as ævoid. I may remark, in conclusion, that the intensity of the ain bears no relation to the size of the growth; but the very mallest sometimes produce as severe, if not severer, suffering s others many times larger.

Stricture of the urethra is the next morbid condition to which I wish to direc tyour attention. It has been described as very rare; perhaps the superlative should be omitted. I have seen many cases of distinct stricture of the urethra. The seat of stricture was near the meatus, and in two appeared to have been the result of some injury or application to the urethra. In all, the symptoms were, as might be supposed, frequent desire to micturate, attended with great difficulty and forcing; a general feeling of bearing down, referred by some to the uterus. The cause of all this was readily detected by examination per vaginam; no disease was found in the urethro-vaginal septum, nor at the meatus. The catheter being passed, great obstruction was experienced; indeed in one only a small conical bougie could be introduced through the stricture. The treatment in these cases was very effectual and permanent—by dilatation by conical bougies, requiring only a few passages of the instrument. It is possible to suppose a more obstinate form of stricture than these—as cicatrices—as the result of injury to the part; but I think it would not be difficult to cure them, like those in the vagina, by stellately scoring with a straight probe-pointed bistoury, and gentle dilatation afterwards. As a rule, we may say that simple strictures of the urethra are not difficult of cure.

Urethritis.—It is by no means a rare, I might almost call it a frequent occurrence, that the urethra becomes affected by inflammation in cases where vaginitis has resulted from either gonorrhœa, simple irritation or injury, or pressure from parturition, &c. It may arise from the irritation of acrid urine, highly charged either with lithates or with ammonia; but the most common of its causes are gonorrhœa and parturition. Sometimes the frequent passage of healthy urine alone, as in irritable bladder, will produce urethritis. In cases of urethritis, simple, chronic, or acute, the pain, by the more intelligent of the sex, is referred distinctly to the water-passage, and to that only; but in as much as occasionally in chronic, and frequently in acute urethritis, the case is complicated with vaginitis, or other abnormal conditions, it is often difficult, sometimes particularly so, to unravel the symptoms, so as to place each to its respective cause. The annoyance complained of by the patient consists generally, not always, of a frequent desire to pass urine; always much pain, scalding or burning, or soreness in the course of the urethra in the act of micturition, lasting for some little time after; sometimes pain in the course of the urethra. The urethra is frequently tender when pressed during the vaginal examination, and a drop or so of purulent mucus may be made to exude from the meatus in certain cases, especially in the acute forms. When the catheter is passed, the patient, in the severer forms, complains much. In the chronic there is always more or less tenderness. In the former, where the vagina is almost always affected, there is general redness of the vulva and vagina ; but in the chronic this has generally subsided, so that only the edge of the meatus and the urethra remains red.

The treatment in the acute form must depend upon the cause, and upon the associated states of vagina, &c. It will be treated in combination with them, by warm, soothing vaginal injections, and by rendering the urine as bland as possible. Where it is the result of delivery, these plans are more particularly to be zealously employed ; and should there be retention of urine also, it will be our duty to pass the catheter, notwithstanding the pain it may cause, otherwise we may have a worse malady as the consequence. I need hardly observe that the use of opiates by the mouth is of great value in these cases.

But in the chronic form we must find other treatment, for it is very persistent, and, although, not seriously affecting health, except when combined with cystitis, yet it becomes very troublesome both to patient and practitioner. I believe we seldom, if ever, find any advantage in the employment of remedies taken by the mouth. In rendering the urine less irritating according to its condition in lulling the irritability of the urethra, we of course shall find advantage, but beyond that my own experience shows none. But by local treatment the disease is as completely under our control as any disease we have to attack. We may employ either astringent solutions, such as alum, or sulphate of zinc solution, or even one of nitrate of silver ; but I think you will prefer the use of these remedies in the more sold form. Nitrate of silver is very efficient, but rather painful ; cast into small sticks, and introduced into the porte-caustique, it can be applied for a short or long period to the whole of the urethra, according as is desirable. It will be generally found sufficient to merely apply it on the withdrawal of the instrument. The most convenient form of porte-caustique is one I use also for the canal of the cervix uteri—a silver tube, having a small caustic-holder on the end of a stilet sliding easily within. It is so constructed that the nitrate can only protrude a very short distance beyond the open end of the tube. The tube is passed up, the nitrate being drawn within. As the tube is drawn out, the nitrate is pushed out, and the mucous membrane is touched lightly throughout. This mode is efficacious, but, at the same time, there is more immediate pain than with some other preparations ; this, however, soon passes off, and in a day or two much relief is obtained. The application should be repeated for about a week, and a third time at the same interval if required.

Perhaps as efficient as any is the use of bougies covered with tannic acid. The best plan is to cover a medium sized gum-elastic bougie with gum-water, and then to dip it into powdered tannic acid. The superfluous quantity is then shaken off, and the film allowed to dry. Before using it, pass it gently through the fingers to remove any roughness that may be present; then dip it into gum-water and pass it up the urethra, and leave it in ten or fifteen minutes. This may be repeated once a week. There is but little irritation generally; perhaps the next day some slight increase; but by three or four days the former irritation has much subsided, and then two or three applications at intervals of a week are sufficient to complete the cure.

Sulphate of zinc cast in sticks, such as you have seen me employ in cases of cervical leucorrhœa, may be used by simply passing the stick up the urethra, or as nitrate of silver in the porte-caustique, as above. We may thus speedily rid our patients of an annoyance which it were vain to expect removed by other than local remedies. I have employed each of these remedies repeatedly, and could instance numerous cases of its satisfactory results. One may suffice.

A lady had, two years since, first of all during her pregnancy, suffered pain in the urethra, with much pain during micturition. This was not alleviated by delivery, but remained constantly annoying her to a very considerable extent; and the dread of passing urine had caused a constant nervous state. She had consulted her medical attendant, without relief. I found the urethra very tender when pressed on her vaginam, and on passing a catheter, she instantly said it touched the seat of her annoyance. I passed a stick of dried sulphate of zinc, allowing it to remain about five minutes. This caused her some pain at the time, which, however subsided in two hours. From this time she had no suffering worth mentioning. A second application was made, and she was quite free from pain when I last heard of her.—*Lancet, Oct.* 12, 1867, *p.* 449.

107.—SORE NIPPLES.

Dr. Blacquieres says, in the *Journal des Connaissances Médicales*, that three or four applications of the following compound cure this complaint:—Cocoa butter, 150 grains; extract of rhatany, ten grains.—*Lancet, Sept.* 21, 1867, *p.* 361.

108.—UTERINE SURGERY IN PARIS.

[The following short article is taken from a report on the present state of surgery in Paris by Mr. Sampson Gamgee, of Birmingham.]

For all examinations and operations on the vagina and uterus Dr. Sims uses the duck-bill speculum, and no other. He dislikes sunlight, and prefers light entering from one window directly at his back to light entering the room from different quarters. The previous evacuation of the rectum greatly facilitates the use of the speculum. A table about four feet long, covered with a blanket, is preferable to a bed or couch for the patient to lie upon. The position is on the left side, body diagonally to the table, so that the buttock rests on the left of the angle next to the operator and the window. The spine straight with the head, which may be raised very slightly if more agreeable; not so the shoulders, for the chest must lie prone, and as flat as possible on the table; neither of the arms under the chest, but well apart. The thighs to be bent at right angles to the trunk; legs at similar angle, an assistant raising the feet slightly. For this purpose a small table, a little higher than the one on which the patient lies, is handy to rest the legs upon. It is convenient to flex the right thigh a little more than the left, so as to bring the right heel just in front of the left ankle. In this position the epigastrium is on a lower level than the pelvis; and great importance is attached to the waist being free from all constriction, so that the abdominal viscera may gravitate freely forwards and downwards, and allow of the entrance of air into the vagina when the speculum is introduced. As already observed, this position is the invariable one for all inspections and operations. But one exception is made. When the patient has cancer of the womb, in order to avoid hemorrhage or other accident from striking the cervix with the speculum, this is introduced while the patient is on her knees on a table, the chest prone and the back concave.

Before using the speculum, Dr. Marion Sims invariably ascertains the size, position, and direction of the womb; and he does this, while the patient lies on her back, by left digital examination, palpation with the right hand above pubes serving to bring the uterus well within reach.

Whenever he introduces anything into the uterus, be it a sound or a tent, a syringe or a knife, he fixes the cervix with a tenaculum, after introducing the speculum, and is thereby enabled to ensure steadiness, and, when necessary, to draw down the neck of the womb into the vagina, and so direct it as at once to see into it. It is only when he so fixes and sees it clearly that he divides the cervix, thus making it an accurately surgical operation, and not merely a mechanical one. In operating for fistula, he always fixes the vaginal wall with a tenaculum before paring it or passing sutures.—*Lancet, Sep.* 7, 1867, *p.* 296.

109.—ON THE DEFECTS OF ORDINARY SPONGE TENTS; WITH AN ACCOUNT OF A NEW KIND OF CARBOLISED SPONGE TENT.

By Robert Ellis, Esq., Surgeon to the Chelsea and Belgrave Dispensary.

The author, after adverting to the serious inconveniences and occasional danger incident to the use of the common sponge tents, proceeded to describe a new kind introduced by himself under the name of carbolised sponge tent. In this invention sponge is still retained as the dilating agent, but the tent is prepared by a peculiar process which renders it incapable of putrefaction, without diminishing its value as a dilator. This is accomplished by introducing into the core of the tent several threads of cotton wick stepped in carbolic acid; and after the sponge is rolled into its proper shape, it is then immersed in cocoa butter to which a certain quantity of glacial carbolic acid is added. The disinfectant properties of this agent completely protect the tents, and they are withdrawn in an inodorous state even after a stay of twelve or eighteen hours in the cervical canal. The shape and size of these sponge tents also differ from the ordinary kind which are both clumsy and dangerous, as well as disgusting, in their use. These are spindle-shaped, and thus accurately adapt themselves to the fusiform character of the canal which they are intended to dilate. They require no support when *in situ*, but, by virtue of the immediate fusion of the enveloping material, they take to their work immediately, and are firmly kept in position. The author stated that he had a large experience of their utility and value; and that they could be procured, ready for use, from Messrs. Bradley.

Mr. Ellis also exhibited an Introducer for Sponge Tents. This instrument consists simply of a slender uterine sound tapered to a fine point, which is thrust up into the tent. A short distance from its extremity a small flat metal collar is attached, on which the sponge tent rests, so as to be firmly supported while it is pressed into its place. Mr. Ellis spoke in high terms of the great handiness of this contrivance, which may be obtained of Messrs. Meyer and Meltzer.—*Lancet, Sept.* 7, 1867, *p.* 295.

110.—A NEW MODE OF VACCINATING RAPIDLY.

Dr. Carenzi, of Turin, has had a silver finger-ring constructed, to which is fixed a little capsule of the same metal. The operator places this ring on the first phalanx of his left thumb, and the capsule being filled with lymph obtained from capillary tubes, he goes on dipping his lancet and vaccinates with great rapidity.—*Lancet, Sept.* 21, 1867, *p.* 361.

111.—ON NIGHTMARE OF CHILDREN.

By Dr. SYDNEY RINGER, Assistant Physician to the Hospital for Sick Children, Great Ormond Street, London.

Violent screaming, which cannot be quieted, and which may last for a few minutes to several hours, is frequently witnessed in children. This is generally produced by one of three causes —hunger, pain, or nightmare. The following is a fair example of a case of screaming from the last cause.

Charles L., 2 years old, came under my care at the out-patient department of the Children's Hospital. The child was badly nourished, and was afflicted with a frequent hacking cough, that troubled him much more at night than day. For two months he had, twice or three times each night, started from his sleep, screaming violently. Each paroxysm of screaming lasted about half an hour. Sometimes he rolled about the bed, threw his arms wildly about, and knocked his head violently against the bed; on other occasions he sat up in the bed and screamed so violently that he became black in the face. While thus afflicted his eyes rolled, and he appeared to be quite unconscious, as he did not recognise his mother, and could not be brought to by her care and attention. His mother stated he did not appear to have his senses. He gave no signs of being in pain. He did not talk, he only screamed violently. After each paroxysm he fell asleep, but his sleep was disturbed, and his eyes still rolled, and he frequently moaned. His gums were neither red nor swollen. His appetite was good, but his bowels had been relaxed for three weeks, and his motions were green and slimy, but no worms were ever seen in them. He was fed judiciously. He wetted the bed at night. During the fortnight that preceded his application for relief at the Hospital, he had suffered from two convulsive fits, when his arms worked and his face twitched. Each of these fits lasted twenty minutes.

Such screaming may occur in children of all ages; but, while it is met with in children of ten or twelve years of age, it is of more common occurrence in those of a few months to two or three years old.

These attacks may last a very variable time, for though the paroxysm may pass away in a few minutes, it sometimes continues for half an hour to one or even two hours. During this time the screaming is violent and continuous. Sometimes the children appear to be asleep throughout the paroxysm, while other children wake from their sleep, but continue to scream with unabated violence; but even when awake they often appear to be unconscious of what occurs around them. They seem to be, as their mothers state, "out of their senses;" thus they for the most part cannot be quieted. Others appear to awake thoroughly, and are then terribly frightened, and often tremble all over. Such children can generally be quieted in a short time by kindness and attention paid to them, but they remain for some time much agitated, and refuse to be left alone, or, if removed from their bed, they are afraid to return to it. Some children cry only a little, but they wake up frightened and trembling.

Such screaming may continue to occur for months and even years, sometimes disappearing for a time, and then, from various causes to be immediately mentioned, it returns again. It is often repeated several times each night for several months.

Such attacks are naturally a source of much annoyance and much anxiety to the parents, and thus medical men are not unfrequently consulted for this affection. The screaming may be so violent that the child becomes "black and blue" in the face, and occasionally it even terminates in a general convulsion. This, however, is unusual, and in my experience occurs only in children who suffer from convulsions without screaming and from other causes. Such paroxysms of screaming sometimes recur only with long intervals. The child may have one attack, but this may not be repeated for some weeks, or even some months. On the other hand, they may recur ten or twelve times a night.

This screaming differs from delirium, as it does not occur in those diseases accompanied by delirium. Moreover, there is no incoherent talking or muttering, while some children can be roused from this state and are then perfectly rational, although greatly frightened; it occurs only during sleep. The mothers often call it nightmare.

The children, the subjects of this affection, are very generally pale, often ill-nourished and out of health. The immediate cause of this screaming appears to be some disturbance of the stomach and intestines. The nature of this affection of the intestinal canal may be very various in different cases, for one child may suffer from constipation while another is troubled with diarrhœa. This disturbance is very generally dependent on food ill-suited to young children; for this irregularity of the bowels, and the screaming which accompanies it, are especially frequent in those children who have been brought up by hand, and who, consequently, suffer on the one hand from diarrhœa, on the other from constipation. Children thus reared suffer, as is well known, very generally from constipation, and pass hard, pale, lumpy motions, sometimes like marbles. These masses may consist of fæces; they are often composed of coagulated undigested milk, of a yellowish or greenish-yellow colour outside, but are white and cheesy within, looking like, and, indeed, being composed of, curds of milk. (It may be here mentioned, in passing, that not uncommonly children pass by the bowel, or sometimes vomit, large masses of the same composition. These are generally two or four inches long, and about an inch in diameter. They often excite much wonder and anxiety on the part of the mother. When broken, the white curdy appearance at ones declares their nature.) Children who suffer from the affection now under consideration are sometimes infested by thread worms, and also show signs of the altered condition of the mucous membrane of the stomach and intestines by itching, heat, and dryness of the inner part of the nose, with itching at the anus. This screaming is increased by anything that interferes with the general health of the child. Thus, it is observed to be worse when the teeth are making their way through the gums, although the irritation and pain which arise from teething appear to be incapable of themselves of exciting this screaming. It is also made worse by slight attacks of catarrh of the lungs, or eruptions on the body. By treatment this screaming can usually be at once arrested. Both general and local treatment are in most cases required, the former to improve the general health, the latter to remove the conditions immediately exciting the screaming.

The diet should be attended to, and any irregularity in the hours at which food is given to the child, or any unsuitability in the nature of the food, must be remedied. Attention to these points will very generally arrest any diarrhœa which may be present, but constipation with hard shotty motions will generally prove more obstinate, for such motions are almost invariably passed by young children under six months old when brought up by hand, and this although they may be correctly fed and take nothing but good cow's milk sufficiently diluted with water. We have seen that these hard, round, lumpy motions are partly composed of coagulated undigested milk. This coagulation in mass can sometimes be stayed by the addition of alkalies to the milk, such as limewater or bicarbonate of soda. The latter is preferable for this purpose, as limewater confines the bowels, and thus bicarbonate of soda should be preferred.

If the bowels are confined, an active purgative will, in the great majority of cases, suffice to stay the screaming, and will insure to the child calm and refreshing sleep. A powder of rhubarb and soda repeated every night, or every other night, till three powders have been given, is useful. If the child be pale, and the constipation recurs and is obstinate, the following prescription will be found very advantageous, namely: Steel wine, to which is added a few drops of tincture of rhubarb, in quantities adapted to the age of the child and to the obstinacy of the constipation. Usually six drops of tincture of rhubarb in a teaspoonful of steel wine given three times a day will open freely and comfortably the bowels of a child from six to nine months old.

In order to effect a permanent cure it is often necessary to give medicines to improve the general health of the child, as these children are frequently pale and badly nourished.

Thus, in children suffering from the affection we have just described, to effect a permanent cure, if the general health be bad, treatment must be directed to the restoration of the body to sound health. In these cases iron, cod-liver oil, with cold sponging prove most useful. Of the various preparations of iron, the tincture of the sesquichloride, in my experience, is decidedly the best. It has appeared to me that bromide of potassium is able to stay this screaming, but as its administration has been accompanied by the use of purgatives, or a regulated diet, it is difficult to determine how far the bromide was useful. It is, however, I feel sure, worthy of a trial in obstinate cases. Cold baths must be given with care; for while they may, if properly administered, do much good, if administered without certain precautions they will do great harm to children. If too great a shock be given to the child, depression of the system will be produced, and this may last even several days after the bath is administered, when the child may be languid and depressed, and may suffer from much chilliness with loss of appetite. Thus the amount of shock produced by the bath must be regulated to the age and strength of the child. In cold sponging of the body the shock caused is proportioned to the coldness of the water and the length of time the bath is continued; while the younger the child, or the weaker its health, the less able is it to bear up against the effect of the shock to which it is exposed. Hence with young children, and especially with those whose system is depressed, the bath should be continued only for a short time, and if the weather be cold, the water must be slightly warmed. When the child is weak, the bath should be continued at first for a few seconds only, and its duration be gradually increased as the child becomes accustomed to its use.

If the following simple plan be adopted, the child, even if very weak, can take the cold bath with advantage, and all chance of depression is removed. The child should be placed before a good fire with its feet in warm water, while the cold water is freely poured over every part of the body except the head and face. The healthy reaction, with the agreeable sensations which follow the use of the bath, may be much increased by placing the child for a short time in the warm bed from which it had just previously been removed. The bath should be given immediately the child leaves its bed, and the breakfast should be taken soon after the sponging is completed.—*Medical Times and Gazette, May* 4, 1867, *p.* 465.

ADDENDA.

312.—DISCOVERY OF A NEW GENERAL ANÆSTHETIC—BICHLORIDE OF METHYLENE.

By Dr. B. W. Richardson, F.R.S.

[Dr. Richardson has discovered a new general anæsthetic, which he for the first time described and exhibited at the late meeting of the British Association.]

The fluid is a bichloride of methylene, its composition being on the new formula CH_2Cl_2. The substance has an odour as sweet as that of chloroform, but it boils at 88° Fahr., whereas chloroform, the terchloride of methylene, requires a temperature of 142° for boiling. The bichloride of methylene rapidly and easily narcotises animals to perfect anæsthesia. It causes scarcely any excitement, and recovery is most perfect. In action it seems to combine the properties of chloroform and of ether, but it is more readily administered than either, and its effects are more permanent.

The bichloride of methylene belongs to the class of chemical organic substances known as the "Monocarbon series." The labours of the modern chemist have enabled him to separate organic bodies, such as this, into certain divisions, greatly to the illustration and simplification of the science. In making his divisions, the chemist recognises as preliminary facts—first, that every body of the organic type is built upon the element *carbon;* and, secondly, that this element possesses the property of combining with itself, by which property it is enabled, by the combinations it makes with other elements, to produce distinct series of compounds, which are classified, in accordance with this rule, into Monocarbons, Dicarbons, and so on. In illustration, let me write down the names of the first five series:

The *Methyl* series. In this the carbon stands as *one*—C.
The *Ethyl* series. In this the carbon stands as *two*—C_2.
The *Propyl* series. In this the carbon stands as *three*—C_3.
The *Butyl* series. In this the carbon stands as *four*—C_4.
The *Amyl* series. In this the carbon stands as *five*—C_5.

And so on up to the melisyl series, where the carbon stands as C_{30}.

The fact of the multiplication of carbon in these series is brought strikingly before us by a simple experiment. Every one of the groups above named, as well as each of the higher groups which we have not followed, possesses an analogous compound called an alcohol. The alcohols, in fact, are those bodies which first come nearest to hand from nature herself, and from which the chemist has commonly started in the course of discovery. We have before us at this present moment the alcohols of five series. We have methylic alcohol, or, as it is commonly called, wood spirit—naphtha; we have ethylic alcohol, the ordinary alcohol of the table; we have propylic and butylic alcohols; and lastly, we have this peculiarly odorous alcohol, the amylic, or fusel oil—potato spirit. I want to show you that in every one of these there is a difference of carbon. I therefore take five ordinary spirit lamps, and trim them with fresh wicks, and charge them respectively with these alcohols. Having done this, I set the lamps alight, and test the different flames. The methylic lamp burns with little light and no smoke. I hold over the flame a white plate, and there is no carbon. The ethylic lamp yields to the plate the faintest trace of darkness, a little carbon that has not been consumed. The propylic and butylic lamps, with their cloudier flames, yield much deposit; while the amylic lamp, burning dull and heavy, its light peering as it were through smoke, yields as much unconsumed carbon as suffices to cover the surface of the white plate in a few seconds.

In combining with other elementary bodies to produce the groups or the families of the organic series of which I have spoken, the carbon forms first an union with hydrogen, by which there is produced a new and basic substance called a *radical*. The radical once-formed compounds can be made upon it just as compounds are made on the bases of the inorganic world; as chlorine, for instance, in combining with the base sodium, produces the compound called chloride of sodium.

The monocarbon group of bodies with which we have to-day to deal affords a good illustration of all these facts in detail. When the carbon in this series combines with three atoms of hydrogen, it produces the radical methyl CH_3, from which various compounds may be made. For sake of clearness, I will place these compounds before you in order and in the tabular form.

Radical methyl, CH_3.

Compounds.

Hydride of methyl (marsh gas) . .	$C\,H_3\,H$
Chloride of methyl	$C\,H_3\,Cl$
Iodide of methyl . . .	$C\,H_3\,I$

Bromide of methyl	CH_3Br
Fluoride of methyl	CH_3Fl
Cyanide of methyl	CH_3CN
Nitirte of methyl	CH_3NO_2
Methyl alcohol	$\left.\begin{matrix}CH_3\\H\end{matrix}\right\}O$

In these compounds you will perceive that the radical methyl remains always the same, CH_3, and all the new compounds, hydride, chloride, and the rest, are made by the direct addition of an element to the radical. The compounds are, in fact, salts of the radical. If I were to describe the the ethyl series or the other series, I should be able to illustrate this same rule, but I must pass now to a further point—viz., to the production of a different series of substances by what is a true change in the construction of the radical itself. The monocarbon series will serve us for one more illustration, and the illustration will bring us, by a natural and easy stage, to the particular substance which is now the object of our learning.

We start, then, with the first compound on the table I have just written—viz., the hydride of methyl, or marsh gas. This, as we have seen, contains the radical methyl in combination with one atom of hydrogen. It may be represented as CHHHH.

In this compound the carbon molecule, or smallest combining part of carbon, is said to be saturated with hydrogen—that is to say, it will combine with no more. But if marsh gas be acted upon by chlorine a change, or rather a series of changes, is induced, leading to the formation of new and distinct compounds: I mean the chlorine can be made to replace the hydrogen or become substitute for it, and, according to the character and extent of the substitution, so is the product. For example:—

(*a*) One part of the hydrogen can be replaced by one of chlorine: then we have the radical methyl CH_3 combining with chlorine, and the result is *chloride of methyl* CH_3Cl.

(*b*) Two parts of the hydrogen can be replaced by chlorine. In this change there is no longer the old radical methyl CH_3, but a new radical *methylene* CH_2, and this new radical is combined with two of chlorine. The resultant compound, therefore, is the *bichloride of methylene* CH_2Cl_2.

(*c*) Three parts of the hydrogen can be replaced by three of chlorine. In this change there is no longer the radical methyl CH_3, nor yet the radical methylene CH_2, but a new radical CH *formyle;* and this new radical is combined with three of chlorine. The resultant compound, therefore, is the *terchloride of formyle* $CHCl_3$, common chloroform.

(*d*) The whole of the hydrogen may be replaced by chlorine when the radicals of carbon with hydrogen are expunged altogether. The resultant compound is the *tetrachloride of carbon* CCl_4.

Thus, step by step, new bodies can be constructed out of marsh gas, from the chloride of methyl to the tetrachloride of carbon; and conversely from the tetrachloride of carbon the changes may be carried back again to the reconstruction of marsh gas.

To keep to our chlorine compounds, we have, then, in view four bodies thus composed :—

C H H H Cl	Chloride of methyl
C H H Cl Cl	Bichloride of methylene.
C H Cl Cl Cl	Terchloride of formyle.
C Cl Cl Cl Cl	Tetrachloride of carbon.

By keeping these brief facts in mind, the position of the bichloride of methylene in this organic series will be easily remembered.

Before I leave these substances as thus grouped together in their chemical place, let me say of them that they all possess the power of producing anæsthesia when they are inhaled as vapours by men and animals. That the two latter of them have this power—I mean chloroform and the tetrachloride of carbon—is well known, but that the two first on the list also exert the same influence is new as a scientific fact. I discovered that chloride of methyl was a certain and gentle anæsthetic in July last, and this led me to hope that something more stable and manageable could be obtained—something that should stand between the chloride and chloroform. That substance is now found in the second body of the group—the bichloride of methylene. That this, like the chloride, would produce rapid, safe, and easy general anæsthesia, I discovered by experiment on August 30 of the present year.

Amongst the various substances which have come before us in what has preceded, there are several other besides the bichloride of methylene which might fairly engage our attention ; but as we have the bichloride for special consideration, I shall diverge only, and that but for a moment, to refer to the chloride of methyl. The chloride of methyl exists in all ordinary temperatures as a permanent gas. As I have already said, it acts as an anæsthetic when given by inhalation. It is very soluble in ether, and when ether is saturated with it the compound fluid is one of the most perfect of anæsthetics. Unfortunately, this compound is not very stable, and therefore it is not so promising a fluid as could be wished. But I believe it to have no equal in the way of evenness of action. The sleep produced by it is rapid, gentle, profound, prolonged, and I found in an experiment, where I may say I forced the animal to die by increasing the quantity of the vapour, that the muscular irritability was perfect one hour and five minutes after death. I notice this fact because, if the more manageable and stable bichloride of methylene should prove unfaithful to experience and to our hopes, we have still this compound remaining for our practice.

One other fact is of interest in reference to the chloride of methyl: it is soluble in water, and water charged with it will take up four volumes. Chlor-methyl water properly prepared is here for your inspection, and I pass it round with a little sugar to render it palatable. You will find it rather agreeable to drink, and I may add that as a drink it is a potent intoxicator. I took half an ounce with a very decided effect, but the influence it exerts is of course very transitory. The fluid might be used with great advantage in medicine as a soothing agent, and as a cooler or refrigerator. At a push this fluid could, I doubt not, be administered to the extent of producing general anæsthesia, and an operation might thus be painlessly performed after a libation instead of an inhalation. Altogether I predict for chloride of methyl a most useful and important place in our remedial treasury; but we will, if you please, now return to the bichloride of methylene, and its physical properties.

The bichloride of methylene is a colourless fluid, having an odour much like the odour of chloroform. It is pleasant to inhale as vapour, and it produces very little irritation of the fauces and air-passages. It boils at 88° Fahr. Its sp. gr. is 1·344. The sp. gr. of its vapour is 2·937; it is, therefore, nearly three times heavier than air.

From its position physically it combines many of the properties of chloroform with those of ether, and these peculiarities must be remembered in its administration. From its easier evaporation it requires more free administration than chloroform, and from its greater density of vapour it requires less in quantity than ether.

There is another physical difference between the bichloride and chloroform to which I would particularly invite your observation. If I take chloroform and diffuse the vapour of it through air in a bell jar thus, I find, when a taper alight is plunged into the jar, that the light is extinguished—in other words, the combustion is stopped. On further inquiry, I also find that the chloroform itself though it has stopped the combustion, has itself undergone no obvious chemical change. We say, therefore, that the chloroform has acted by a catalytic process: it has stopped oxidation by its mere presence without undergoing decomposition. I take next the bichloride of methylene, diffuse that in vapour through the jar, and plunge in the lighted taper. And now see the difference: the vapour burns in a brilliant flame, filling the jar. Here I have decomposed the substance; the carbon has been turned, by union with oxygen, into carbonic acid, and the hydrogen and the chlorine have been turned, by their new union, into hydrochloric acid. The proof of this latter fact concludes a singularly pretty experiment; I pour a few drops of strong ammonia into the jar in which the bichloride of methylene has been burned, and I produce a dense cloud of chloride of ammonium in white vapour which pours out of the jar like water. I have been careful in showing these experiments with chloroform and bichloride of methylene, and the different behaviours of the latter in the presence of flame, because the experiments bear on one of the most able and ingenious theories ever put forward to explain the action of anæsthetics on living organisms. Some of you will know that I refer to the theory of Dr. Snow. Snow, observing that the vapour of chloroform extinguished flame, as we have just seen, reasoned that, as it thus stopped the combustion of a taper, so by its catalytic action it stopped the combustion of blood, from which arrest all the after phenomena of anæsthesia took their origin. "I could demonstrate all the phenomena of anæsthesia on a farthing candle," was one of his striking epigrams. But here we have a true anæsthetic, which will burn readily, giving brisk combustion. This fact, in so far as it goes is not in accordance with the theory of my late distinguished friend, and in pointing out the fact I do no more in correction than I should for a favoured theory of my own or than he would were he here to speak for himself.

The bichloride of methyl mixes readily and well with absolute ether, and as the two fluids have nearly the same boiling point —four degrees of temperature being the extreme of difference —when they are combined they form a compound which vaporises evenly and equally. The difference in the specific gravities of the two vapours is the only objection to the combination. The bichloride further combines with chloroform in all proportions.

One more physical matter in respect to the bichloride of methylene, and this part of the subject may be concluded. The fluid should have at all times a neutral reaction to test-paper. If it show any acidity, there is present a trace of hydrochloric acid, and the vapour, which under such circumstances would also contain the acid, would be irritating to the throat, and perhaps dangerous to life. The presence of acid in the fluid is not a very probable accident—not more probable than the presence of chlorine in chloroform—but it might occur, and it ought therefore to be known. To prevent decomposition, the bichloride of methylene should be kept like chloroform, well guarded from the action of sunlight.

[To show the relative effects of bichloride of methylene, chloroform, and tetrachloride of carbon, Dr. Richardson placed three pigeons in glass jars, and caused each of them to inhale the vapour of one of the above-named anæsthetics. The pigeon to which chloroform was supplied was the first to become insensible ; on being removed, and placed on the table, it gradually recovered in a few minutes. The bichloride of methylene was longer in producing anæsthesia; but the duration of the insensibility was longer, and the animal recovered more suddenly than the other. The production of anæsthesia with the bichloride of methylene appeared to be attended with less acceleration of the respiration than when chloroform was used. The animal treated with tetrachloride of carbon remained unaffected for a much longer time than the others, owing to the high vaporising point of the compound and the coldness of the atmosphere. The vapour, when it at last began to act, produced some sickness, which was followed by anæsthesia.]

There is one effect connected with the bichloride which I regret to have to record : the bichloride of carbon in some instances produces vomiting ; whether in the human subject that very unpleasant symptom will be less frequent than after chloroform, experience only can decide. Owing to the readiness with which the bichloride finds its way out of the organism, it is probable that the act of vomiting, when excited by it, will be less severe and less prolonged, but beyond this it would be unsafe to speak. The fact that vomiting is produced is all that is positive, and it is a misfortune that must not be concealed.

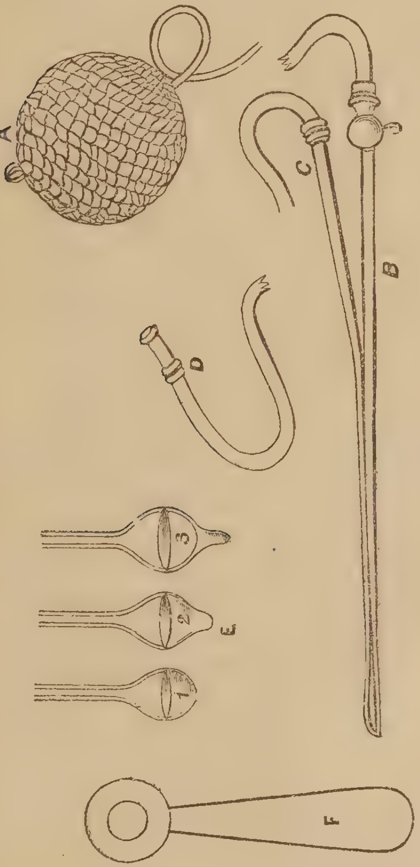
On animals the bichloride of methylene acts more evenly on the respiration and the circulation than any other anæsthetic with which I am acquainted. When the breathing is quickened the pulse also is quickened in proportion, and when the breathing is slow and tranquil the pulse is the same. This is a good point, because there is no condition more perilous than disturbed balance of the circulating and respirating systems. The equality of action depends on equality of diffusion of the agent through the nervous centres, an equality evidenced also in the general ease of the transition of the subject from the first to the third degree of narcotism, and in the suddenness and completeness of the recovery when that once commences.

On Administering the Bichloride of Methylene to the Human Subject.—When I had learned by repeated experiments, that the bichloride of methylene could be safely administered to inferior animals, I inhaled it myself until it produced insensibility. I found the vapour very pleasant to breathe, and little irritating, while drowsiness came on and unconsciousness without any noise in the head or oppression. I recovered also as the animals seemed to recover—at once and completely. I felt, in fact, as though I had merely shut my eyes and had opened them again. In the meantime, however, I had performed certain acts of a motor kind unconsciously; for I inhaled the vapour in the laboratory, and there went into sleep, but I awoke in the yard adjoining. This was on September 28th last. I inhaled on the occasion from a cup-shaped sponge. Since then, I have inhaled the vapour in smaller quantities from several instruments with the effect of proving that there is little difference required for administration between the bichloride and chloroform. The bichloride may be inhaled from a moist sponge, from a balloon or Clover's bag, or from a funnel such as this, made by stretching a few layers of thin cloth over a small wicker work such as is used for surrounding a flower pot, and which opens or closes to any size that may be required. A metallic inhaler such as Snow's chloroform inhaler presents too small a surface for evaporation. In the course of administration, a little more bichloride of methylene is required in the earlier stages than would be required if chloroform were being used, the fluid being more easily vaporisable, and the loss greater. One drachm of bichloride to forty grains (or, in common phrase, minims) of chloroform is a fair statement of the difference required; but when the narcotism is well set up, less of the bichloride is needed, in animals, to sustain the effect, the repetition of the administration not been so frequently demanded.

The Destructive Power of Bichloride of Methylene.—Like all anæsthetics given by inhalation, the bichloride of methylene has the power to destroy life. Its safety as a general anæsthetic must, therefore, be accepted as relative rather than absolute. I have tried to ascertain its relative value with as much care and candour as I could summon, and the result of my work leads me to hope that the balance of safety is on the side of the bichloride. Three observations bring me to this reasoning. First, I find that if two animals of the same age and kind, say pigeons, be placed in chambers of the same size, and exposed at the same temperatures, and under other conditions the same, to equal values of chloroform, tetrachloride of carbon, and bichloride of methylene, the resistance to death will be as fourteen to five in favour of the bichloride of methylene against the tetrachloride of carbon, and as fourteen to nine against the chloroform.—*Medical Times and Gazette, Sept.* 7, *Oct.* 19, *and Nov.* 2, 1867, *pp.* 259, 423, 479.

113.—THE BLOW-PIPE GAS CAUTERY.

At the last meeting of the Royal Medical and Chirurgical Society, Mr. Alex. Bruce exhibited an instrument lately invented by himself (of which we give a woodcut) in which the intense heat of the blow-pipe flame is made available as a cauterising agent. The instrument essentially consists of two tubes, about six inches in length, one of which serves as a burner, and is



Mr. Bruce's Blow-Pipe Gas Cautery.

A, India-rubber ball to receive the supply of gas. Represented proportionally much smaller than natural.
B, Tube connected with the receiver: the burner.
C, The blow-pipe tube. D, The mouth-piece.
E, The platinum cups of three different shapes. Represented proportionally larger than natural.
F, One form of shield.

capable of being attached to an india-rubber ball containing the requisite supply of gas, this receiver being easily filled from any ordinary gas-jet by means of one of Dr. Andrew Clark's pump-balls; the other tube is attached to the former somewhat obliquely, so that a stream of air blown through it by the operator impinges upon the flame, and converts it from one of rounded form and low intensity into a sharply-pointed conical jet, possessing sufficient heat to fuse the ordinary metals. This may be applied directly to the part, or it may be employed to heat a small platinum cup, which is so attached to the extremity of the tube as to receive the flame, and which becomes almost instantaneously of a bright red-heat. The cups are made of various shapes, according to the uses to which they are to be applied, the three most useful forms being the hemispherical, the conical, and the pointed. Shields of various sizes may also be used to protect the surrounding parts during the application of the naked flame or of the platinum points in cases in which it is desirable to limit the action as much as possible. The whole apparatus is packed in a small box, and has the advantage over the galvanic battery both in portability and cheapness. The instrument was made from Mr. Bruce's design by Mr. Baker, of Holborn, who, we are informed, will shortly be prepared to supply them to the profession.—*Medical Times and Gazette, June* 1, 1867, *p.* 598.

114.—AN ANODYNE FORMULA.

The following formula is recommended for combining chloroform and morphia for internal administration. One part, by weight, of morphia is dissolved in two parts of rectified wine-vinegar and twenty parts of rectified spirit of wine; and the solution, when cold, is mixed with eighty parts of chloroform. One drop contains the three-hundredth part of a grain of morphia. The dose for a child is two to fifteen drops; for an adult, thirty to forty drops. It is said to give relief in most painful affections much more quickly and certainly than morphia alone, and to leave none of the unpleasant after-effects of opium. The subcutaneous injection of morphia during chloroform narcosis, is strongly advocated in all those cases where it is desirable to maintain the state of unconsciousness for a lengthened period.—*British Medical Journal, April* 13, 1867, *p.* 422.

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